PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

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NINETY-FIFTH SESSION.

Monday, 7th January 1878.

The Right Rev. BISHOP COTTERILL, Vice-President, in the Chair.

The following Communications were read :—

1. On Gladstone's Theory of Colour-Sense in Homer. By Professor Blackie.

2. Note on a Geometrical Theorem. By Prof. Tait.

In "Trans. R.S.E." (1864–5) Fox Talbot proved very simply, by means of a species of co-ordinates depending on confocal conics, the following theorem, at the same time asking for a simple geometrical proof.

If two sets of three concentric circles, with the same common difference of radii, intersect one another—the chords of the arcs intercepted on the mean circle of each series by the extremes of the other are equal.

A properly geometrical proof may possibly be obtained by showing that the *middle points* of these arcs are equidistant from the line joining the centres. It is, of course, quite easy to build up a quasi-geometrical proof, but Talbot's would be much better.

The following investigation shows the nature of the theorem, and gives some elegant constructions.

Let d be the common difference, b and c the mean radii, and a the distance between the centres. Then the square of one of the chords is easily seen to be

$$p^2 = 2c^2\left(1 - \cos\left(\theta' \pm \theta\right)\right),$$

where θ' and θ are given by

$$(b-d)^2 = a^2 + c^2 - 2ac\cos\theta\,,$$
$$(b+d)^2 = a^2 + c^2 - 2ac\cos\theta'.$$

The expressions for the other chords differ only by the interchange of b and c. Elimination gives at once

$$\begin{aligned}
p^2 = 2c^2 \Bigg\{ 1 - & \frac{a^2+c^2-(b-d)^2}{2ac} \cdot \frac{a^2+c^2-(b+d)^2}{2ac} \\
& \mp \left\{ 1 - \left(\frac{a^2+c^2-(b-d)^2}{2ac} \right)^2 \right\}^{\frac{1}{2}} \left\{ 1 - \left(\frac{a^2+c^2-(b+d)^2}{2ac} \right)^2 \right\}^{\frac{1}{2}} \Bigg\} \\
= \frac{1}{4a^2} \Big\{ & 4(a^2+d^2)(b^2+c^2) - 2(b^2-c^2)^2 - 2(a^2-d^2)^2 \\
& \mp 2\left(4a^2c^2 - \left(a^2+c^2-(b-d)^2\right)^2\right)^{\frac{1}{2}} \left(4a^2c^2 - \left(a^2+c^2-(b+d)^2\right)^2\right)^{\frac{1}{2}} \Big\} \\
= \frac{4}{a^2} & \left(\mathrm{A}^2 + \mathrm{A}'^2 \mp 2\mathrm{A}\mathrm{A}' \right)
\end{aligned}$$

where A and A′ are the areas of the "inscribable" quadrilaterals, crossed and uncrossed, whose sides are a, b, c, d. This, of course, proves Talbot's theorem.

Hence

$$p^2 = 4\,\frac{(\mathrm{A}' \mp \mathrm{A})^2}{a^2}\,,$$

a remarkably simple expression. The two values of p are given at once by Talbot's diagram, and the rectangles under their quarter sum, and difference, respectively, with the distance between the centres, give the areas of the quadrilaterals above mentioned. Or, better, the triangles whose angular points are the middles of the arcs respectively, and the centres, have areas equal to half the sum and half the difference of the quadrilaterals.

The symmetry of these expressions shows that in Talbot's theorem any two of the four quantities employed may be interchanged—the lengths of the corresponding pairs of equal chords being always inversely as the quantity chosen for the distance between the centres of the two series of circles.

Again, it is easy to see that we have by the above equations

$$A' = ac \sin\frac{\theta'}{2}\cos\frac{\theta}{2},$$

$$A = ac \cos\frac{\theta'}{2}\sin\frac{\theta}{2},$$

so that, construct the figure how we will with four given lines, the ratio of the tangents of the halves of the pair of angles corresponding to θ, θ', is constant. This is the relation between True and Excentric Anomaly. And we have also the very simple expression

$$AA' = \frac{a^2c^2}{4}\sin\theta\sin\theta',$$

so that the product of the areas of the crossed and uncrossed quadrilaterals is equal to the product of the areas of the (construction) triangles whose sides are

$$a, c, b-d,$$

and $$a, c, b+d,$$

respectively. Here again the letters may be interchanged at will; which, in itself, is a curious theorem.

While seeking a quaternion proof of the above theorem, I hit upon the following result. Given two opposite sides of a gauche quadrilateral in magnitude and direction. If one of these be fixed, and if the diagonals are to be of equal lengths, the locus of either end of the other is a plane.

Professor Tait, in consequence of the lateness of the hour, postponed his paper "On the Strength of the Currents required to work a Telephone." He said that the title given in the billet did not fully describe the contents. These referred not only to various measurements of the actual currents employed, whether produced from a cell or a Holtz machine, or by induction, but also to the mode in which the sounds are reproduced. He stated that he believed it would soon be possible to employ the instrument for the study of internal changes of form in all bodies, and also that in its construction magnets might be entirely dispensed with. He also stated that Mr James Blyth had with success substituted a copper plate for the iron disc in the receiving telephone, and had found that even wood, vulcanised india-rubber, tinfoil, and paper, might be employed for the same purpose—though not with very good results. In the transmitting instrument Mr Blyth had managed to employ a block of iron 8 inches thick.

Professor Tait also exhibited a double mouthpiece, by means of which it is easy for two players to produce chords from a French horn.

The following Gentlemen were elected Honorary Fellows of the Society:—

Otto Struve, St Petersburg.
Professor J. N. Madvig, Copenhagen.
Professor Balfour Stewart, Manchester.

The following Gentlemen were duly elected Fellows of the Society:—

W. H. Allchin, M.B. (Lond.), M.R.C.P., Wimpole Street, London.
Richard Norris, M.D., Professor of Physiology, Queen's College, Birmingham.
Daniel John Cunningham, M.D., 9 Gladstone Terrace.
Alan Macdougall, Memb. Inst. C.E., North British Railway, Millburn Tower, Corstorphine.
John Frederick Bateman, F.R.S., V.P. Institution C.E., 16 Great George Street, Westminster.
William King, M.A., Stewart Villa, Dean.
P. R. Scott Lang, B.Sc., St Ronan's, Viewforth.

Monday, 21st January 1878.

Sir WILLIAM THOMSON, President, in the Chair.

The Canon of Sines for each 2000th part of the quadrant, to 33 places, and true throughout to the thirtieth figure, by Edward Sang, was laid on the table.

The following Communications were read:—

1. On the Tabulation of all Fractions whose values are between two prescribed limits. By Edward Sang.

2. Can the Law of Multiple Proportions be demonstrated from Analytical Data? By W. Dittmar.

3. On the Solid Fatty Acids of Coco-Nut Oil. By G. Carr Robinson.

4. Suspension, Solution, and Chemical Combinations. By William Durham.

Some time ago I made some experiments on "Suspension of Clay in Water, and in Acid and Saline Solutions." These formed the subject of several communications to the Royal Physical Society of Edinburgh, and were afterwards published in the "Chemical News." I shall describe some of these experiments as an introduction to this paper.

1st. *Clay in Water.*—A few grains of fine white clay were stirred up with about a pint of pure water in a glass jar. The time which the water took to clear and the clay to deposit itself in the jar was noted, and found to be about 30 to 36 hours.

2d. *Clay, Water, and Acid.*—To a similar quantity of clay and water were added a few drops of acid (various acids were tried, with the same result in each case), and the time the liquid took to clear again noted. In this case, the time was from half an hour to one hour, according to the quantity of acid used. So sensitive was this action, that when the water was just touched with a glass rod dipped in sulphuric acid, the time of clearing was greatly shortened.

3d. *Clay, Water, and Salt.*—In place of acid, salts of various kinds (including common salt ($NaCl$)) were added to the clay and water, and the effect in every case was to shorten the time of precipitation of the clay and clearing of the water, according to the kind and quantity of salt used. As bearing on the precipitation of salt at the mouths of rivers, I may mention that the water taken from the end of Leith pier was about the best mixture for precipitating the clay quickly.

4th. *Clay, Water, and Alkali.*—The alkalis (potash, soda, and ammonia) were next tried with the clay and water, and when added in very *small quantities* instead of hastening the precipitation of the clay, like the acids and salts, they retarded it so that in some cases it was 90 hours before the liquid was clear. In larger quantities they acted like the acids and salts.

In endeavouring to ascertain the cause of these phenomena, I was led to experiment on various solutions, and obtained results which appear to me very interesting, and to open up a line of research likely to lead to important results as to the laws of solution and chemical affinity. The following are the more important facts as yet determined:—

1. *NaCl and HCl.*—To a saturated solution of common salt (NaCl) was added some strong hydrochloric acid (HCl). Salt was rapidly precipitated on each addition of acid. This action of HCl is usually described by saying that salt is insoluble in hydrochloric acid, but the mode of action does not seem to have attracted attention. The following experiments throw some light on the matter.

2. $Na_2SO_4 + IOH_2O$ *and HCl.*—To a saturated solution in water of sodium sulphate was added strong solution of hydrochloric acid. *Anhydrous* sodium sulphate was quickly precipitated. This precipitate was quickly dissolved on the addition of a further quantity of HCl. In this case it cannot be said that anhydrous sodium sulphate is insoluble in hydrochloric acid, because it is dissolved by it when added after its precipitation. The action is exactly analogous to what occurs when ammonia is added to a salt of zinc; the ammonia first combines with the acid of the zinc salt throwing down a precipitate of zinc oxide, then, on a further quantity of ammonia being added, the zinc oxide is dissolved. In like manner, in the case of solution of sodium sulphate the HCl first combines with the water and precipitates the anhydrous salt, and then, by a further addition of acid, the salt is again dissolved. This is made more clear by the next experiment.

3. *Crystals of* $Na_2SO_4 + 10H_2O$ *and HCl.*—Strong hydrochloric acid was poured over some undissolved crystals of sodium sulphate. Rapidly the crystals were broken up, the water uniting with the acid, and the anhydrous salt left, which, as in the former case, was dissolved by the addition of more acid.

4. $CaCl_2$ *and HCl.*—To a saturated solution in water of calcium chloride was added some strong hydrochloric acid. No action was apparent. As calcium chloride has a strong affinity for water, I concluded that the affinities were balanced in the two solutions, and therefore there was no action. To upset this balance, if it existed, I added to the hydrochloric acid solution some fragments of solid calcium chloride, which, as anticipated, rapidly dissolved with effervescence, expelling hydrochloric acid gas copiously. Further,

5. A weighed quantity of solid calcium chloride was dissolved in a measured quantity of water, and the rise in temperature noted. This was 15° Fahr.

6. The same quantity of calcium chloride was dissolved in strong hydrochloric acid and the rise in temperature also noted. This was 6° Fahr., being a difference of 9°, accounted for, no doubt, by the vapourising of the hydrochloric acid gas expelled in the latter case.

7. *$NaCl$ and $CaCl_2$.*—Calcium chloride, both in solution and solid, was added to saturated solution of common salt, and in both cases the sodium salt was precipitated, the calcium chloride taking its place in the solution.

8. *HCl and H_2SO_4.*—Strong sulphuric acid was added to strong hydrochloric acid, when the latter as gas was expelled with effervescence.

9. *$CaSO_4HCl$ and H_2SO_4.*—Strong sulphuric acid was added to a saturated solution of calcium sulphate in hydrochloric acid, when calcium sulphate was precipitated, and hydrochloric acid gas expelled.

I next looked about for two solvents which would dissolve the same substance, and yet precipitate it when mixed. These results I found with sulphuric acid, water, and calcium sulphate.

10. *$CaSO_4$, Water, and H_2SO_4.*—To a saturated solution of calcium sulphate *in water* was added some strong sulphuric acid. On cooling, the calcium sulphate was precipitated.

11. *$CaSO_4$, H_2SO_4 and Water.*—To a saturated solution of calcium sulphate in *sulphuric acid* was added water. As before, on cooling, calcium sulphate was precipitated.

12. Two solutions (saturated) of calcium sulphate, one in water and one in sulphuric acid, were mixed when calcium sulphate was precipitated on cooling.

Three experiments similar to Nos. 10, 11, and 12 were performed, only substituting clay in suspension for calcium sulphate in solution, and the results were similar. Clay, in sulphuric acid (strong), was suspended nearly as long as in pure water, but on mixing the liquid, or on adding sulphuric acid to the water, or water to the sulphuric acid, the precipitation of the clay was greatly accelerated.

I hope to extend these experiments to other substances than those already operated upon; to enter more minutely into the various phenomena; and to determine the law of what I may call "Solution Equivalents." To know, for instance, the relation between the quantity of calcium chloride dissolved and the quantity of common salt precipitated or hydrochloric acid gas expelled from solution, and various other points which suggest themselves for investigation.

The experiments, however, so far as they go, seem to point to certain conclusions which are interesting and suggestive.

1st. There seems to be a *regular gradation of chemical attraction from that exhibited in the suspension of clay in water up to that exhibited in the attraction of sulphuric acid for water which we call chemical affinity.* The attraction of clay for water is not so strong as the attraction of salts which are dissolved in water. Then again, the attraction of salts is not so strong as that of hydrochloric acid, which almost forms a definite chemical compound with water. Then, finally, we reach sulphuric acid with the strongest attraction of all, and forming more than one definite chemical compound with water, and easily displacing from their combinations with water hydrochloric acid; salts it does not decompose and clay in suspension.

2d. That *chemical combination, solution, and suspension diffe only in degree, and are manifestations of the same force.* The few drops of sulphuric acid added to the water with clay in suspension attracts and holds the water with the same force as a salt in solution and precipitates the clay in the same manner, and as the water is evaporated increases its hold gradually on what remains until it is strong enough to form definite chemical compounds.

3d. *The attraction of chemical affinity is not, in all cases at any rate, exhausted when a definite compound is formed, but has sufficient power left to form solution or suspension compounds.* Thus calcium sulphate is a definite chemical compound, but it still possesses sufficient affinity for sulphuric acid to enter into solution with it. This view would explain the researches of M. Stus on atomic weight. He proved that Prout's law, that the atomic weights of the elements are simple multiples of that of hydrogen, is not correct, though very nearly so; the differences being very small fractional. If it be true that the attraction of affinity is not entirely exhausted in a definite compound, but that there is a fraction of it, so to speak, to spare, we should only find Prout's law to hold good if we could analyse only one molecule of any compound, but, as in any analysis we can make, there must be many molecules, such atom of the molecules having a fraction of its affinity for the other to spare, these fractions would unite and hold in combination an extra number of the other atoms, not so firmly, perhaps, but still firmly enough to make the whole appear a definite compound on analysis, and this would affect the calculation for atomic weight. Thus, suppose two atoms of Cl = 71 combine with one atom of Ca = 40 and still have $\frac{1}{100}$ part of affinity to spare, then 200 atoms of Cl would take up 101 atoms of Ca, and from this analysis we should make the atomic weight of Ca not 40 but 40·4.

4th. If chemical combination and solution are due to the same force, then solution will loosen the combination by spreading the affinity, and possibly there may be a re-arrangement of the soluble and solvent analogous to what is known to take place when two salts are mixed having different acids and bases. Hence the powerful effects of solution in promoting chemical reaction and electric conductivity.

5th. A point of practical importance may be noted regarding analysis. Many substances are added indefinitely to solution to render insoluble some body held in solution, which quantity is to be estimated. Now, if the way in which one substance renders another insoluble is by combination with the solvent, it is quite clear that if either too much or too little be added to the solvent, an error may be made in the analysis, as the whole of the precipitated body may not be thrown down.

6th. A further investigation of this subject may throw some light on the manner in which the solubility of a solid in a liquid is related to the chemical composition of the two.

5. Note on the Surface of a Body in terms of a Volume-Integral. By Professor Tait.

In § 25 of my paper on *Green's and other Allied Theorems* ("Trans. R. S. E." 1869–70) I gave the following relation between a volume and a surface integral, the limits being determined by any simply continuous closed space :—

$$\iiint \nabla\tau d\varsigma = \iint U\nu \, \tau ds.$$

If in this equation we assume τ (which is arbitrary) to be equal to $U\nu$ at every point of the surface, we have

$$\tau = U\nu = U\nabla P$$

where $P = C$ is the (scalar) equation of the surface. The equation then becomes

$$\iiint \nabla U(\nabla P) d\varsigma = -\iint ds.$$

Applied to the ellipsoid—

$$\frac{x^2}{a^2} + \frac{y^2}{b^2} + \frac{z^2}{c^2} = 1 \, ,$$

this gives for the whole surface the expression—

$$\iiint dxdydz \frac{\frac{x^2}{a^4}\left(\frac{1}{b^2} + \frac{1}{c^2}\right) + \frac{y^2}{b^4}\left(\frac{1}{c^2} + \frac{1}{a^2}\right) + \frac{z^2}{c^4}\left(\frac{1}{a^2} + \frac{1}{b^2}\right)}{\left(\frac{x^2}{a^4} + \frac{y^2}{b^4} + \frac{z^2}{c^4}\right)^{\frac{3}{2}}}$$

the limits being given by the equation of the surface.

6. On a White Sunbow. By Sir Robert Christison, Bart.

As the phenomenon of a colourless rainbow, which was seen here in the forenoon of Thursday the 10th January, seems to be very rare, never having been witnessed before either by myself or by any of my friends to whom I have mentioned the subject, I beg to offer the Society the following description :—

On my way that forenoon to the Botanic Garden, and arriving about a quarter-past eleven at the open view of the north at the bottom of Pitt Street, my attention was arrested by the appearance of a magnificent white bow, visible in its entire arch from end to end in the northern sky.

The air was frosty, very dry, uncommonly still, and in most quarters moderately clear. The smoke of the Old Town, however, rising high in the stillness above the ridge of the High Street to the south, obscured greatly the sun, which shone through the upper region of the smoky veil without the slightest definition of its disc, white nevertheless, but so shorn of its brightness that I could easily look it in the face. I was unable to detect anywhere the slightest appearance of a shower or rain-cloud. The sun, my place of observation, and the summit of the arch, were in the same vertical plane. The summit of the arch reached about half-way to the zenith. The northern sky on which it was formed was somewhat hazy and grey in its lower region, but blue-grey, and tolerably clear in the region of the upper two-thirds of the arch.

In form the arch was identical with that of an ordinary rainbow, except that I thought it considerably broader; and its edges were in many places somewhat broken, so that it had exactly the appearance as if the sun had gathered in an arch a number of little woolly cloudlets. On minute search I could not detect any trace of colour from end to end. I asked the opinion on this point of two gentlemen whom I met at the lowest part of the road, at the wall between the road and the river, and one of them thought he could detect a very faint trace of colour over a small space at the extremity of the western limb. As the absence of colour, however, was the main phenomenon, I scanned the whole curve again and again with great attention, but could see no coloration anywhere.

At this lowest point of the road the edges of the bow were seen much more defined and sharp than when I first noticed it. As I advanced up the gentle slope from Warriston bridge towards the Botanic Garden, the summit of the arch began to break up, and to present the appearance of irregular flimsy cloudlets ascending in the sky above it. But before reaching the Garden gate the whole arch again formed an unbroken bow, and with both edges sharply defined like those of a common rainbow. At the same time a similar secondary arch had begun to form below the principal one, only half its width, and much closer to its neighbour than I remember to have seen in a double rainbow of the ordinary kind.

On returning homeward, about fifteen minutes later, I still observed, on issuing from the Garden, a sharply-defined colourless principal bow, and now a complete secondary one under every part of the bow visible from the roadway. As I proceeded southward every now and then the upper region of the bow seemed to be breaking up, and this appearance was very marked when I reached

Heriot Row, at the head of Dundas Street, the highest station in my walk. As I went westward along Heriot Row the breaking-up appeared greater and greater at every interruption of the street, which gave me a view of the northern sky; and when I reached the landing-place of my house, in Moray Place, from which, however, only the western half of the region of the bow could be seen, the whole appearances had vanished, and the sky was everywhere mottled with thin grey fleecy clouds, small and of irregular ill-defined outline. I did not again look for it, but I understand it was partly seen by others so late as two P.M.

Various particulars, which it is unnecessary to mention, led me to suppose at the time that this colourless bow had some connection with the smoky column of air through which the sun's rays penetrated. But this supposition was put an end to by learning from my son that, when at Craigiehall, five miles west from town, in a smokeless atmosphere, he observed the bow distinctly about one o'clock. Its edges were never sharply defined so long as he noticed it. But it had no colour. Another gentleman present thought there was a limited blueness at one place. But my son satisfied himself that this was owing to a patch of blue sky behind, and he is sure that there was no colour at any part of the bow visible to him.

A better explanation has been suggested to me by Professor Tait, to whose theory I subscribe. But I leave it to himself to explain his views.

By Professor Tait.

I was unfortunate in not seeing the phenomenon till nearly 2 P.M., when I was on my way from College to the Observatory. It was then very faint, but I saw at once that it differed in a marked manner from an ordinary rainbow. From what I could see, I attributed its apparent whiteness to the greatly increased *effective* surface from which the light came. This was probably due to reflection from ice-crystals mixed with the drops of water in the thin strata of cloud which covered the whole sky. The sun's light was much dimmed, and the edge of its disc was very indistinct, as the clouds immediately round it, to the distance of at least a diameter, appeared nearly as bright as the disc itself. Hence this rainbow was probably very much less pure, while also much less bright, than the usual one.

This suggestion seems fully to explain all the appearances which I saw. It will be observed that both Sir R. Christison and Mr Buchan noticed the peculiar brightness of the clouds near the sun, so that it is probable that this explanation applies to the phenomenon as seen by them also. The peculiarities noticed in the position of the spurious bows (when seen) are of course dependent on the actual size, as well as the greater or less uniformity of size, of the drops which produced the rainbow; and these may well have been exceedingly variable as regards both time, locality, and height in the atmosphere.

[*Added, April* 8.]

I find that this nearly colourless rainbow is very easily reproduced in my class-room, when the sunlight employed to form a rainbow in fine spray is made to pass first through a large vessel with parallel glass sides, containing water and a little milk. This arrangement imitates very closely the circumstances of the 10th January.

By J. Christison, Esq.

I saw it first at Craigie Hall, five miles west of Edinburgh, about 11.30 or thereby. At first it did not make much impression, as the house front runs east and west, and I was standing at the front door, and consequently only saw half of the bow, and took it to be an accidental arrangement of the light clouds that covered most of the sky. By and by the idea of its being a rainbow struck me, and a move out from the house showed the full bow. I noticed it off and on for about an hour. Sometimes it was indistinct, but generally it was evident enough to attract attention at once. I did not, however, at any time see anything like clear definition of outline. There was always a sort of wavy indistinctness.

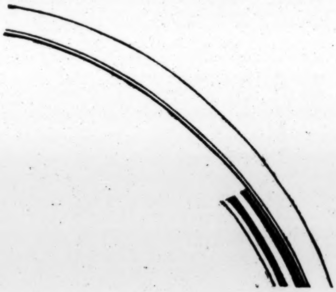
As to colour, I tried hard to convince myself I made it out, but without effect, and am quite satisfied there was none at any of the periods I looked at the bow. One of the party at Craigie Hall thought he made out colour, but on his pointing out the part where he thought it was visible I was satisfied that it was only a break in the bow, with a bit of very light blue sky showing through. I did not notice it after 12.30 or 12.45, as I was indoors.

It was pretty hard frost all the time. The only effect the sun had was to disperse a little of the hoar-frost, which was thick and heavy out there, and to make a few of the flat stones among the gravel show damp. I noticed a thermometer in the garden standing at 32. It was well placed for shelter from the sun; but it was within a few feet of two brick walls, one of them the end wall of a forcing-house. Could that affect the thermometer? The ground—grass, gravel, and earth—remained hard all day. I noticed neither rain nor snow.

The only local peculiarity on the occasion that I can remember was that the gardener was burning rubbish, off and on, all the forenoon between us and the bow. There was no smoke that I noticed after eleven, but there must have been a good deal of heat rising from the red ashes. This was close to the house, say 100 yards at most. The bow had all the appearance of being distant; but had it not been seen here (and no doubt elsewhere) there might have been room for suspicion of this appearance being a deception perhaps.

By Mr Buchan.

The rainbow described by Sir Robert Christison at meeting of the Royal Society was observed by us from the north windows of the Scottish Meteorological Society. The above is a rough sketch of it made at the time.



The eastern limb of it rested on the tops of the houses of Leith Street, against what appeared to be a smoke-like cloud, precisely similar to what often accompanies the aurora. It was on this portion only of the phenomenon that the following points were noticed:—

1. *Spurious Bows.*—Two such bows, very distinctly marked, were seen within the primary bow. The first spurious bow was separated from the primary by an intensely black band the width of the spurious bow itself, while this spurious bow and its black band were together equal in breadth to the primary bow. The second spurious

bow was separated from the first spurious bow by an intensely black band the width of the second spurious bow, and both second spurious bow with its black band were together the width of the first spurious bow.

2. *Colour.*—Colours were seen on the portion of the limb described above as standing out against the black smoke-like cloud. The colours, though far from being well pronounced, were distinct. Miss Buchan stated she saw the colours as we looked at the bow through the window, which at the time stood in need of the window-cleaner; but I was unable to detect the colours till the window was thrown up, when they were distinctly visible.

In no other part of the bow was any development of colour visible, either when I first observed it in St Andrew Square, or afterwards at various times in the office.

The frost was keen at the time, but it is probable that a S.W. wind with thaw had then set in aloft. I understand from Mr J. Gibson-Thomson, of York Place, who called at this office shortly thereafter, that while driving out to the country in the afternoon the wind had shifted into S.W., and the tops of the mud-ridges on the road had become soft with the thaw which had set in.

The width of the primary, or its visible portion, appeared to be about a third narrower than the ordinary coloured rainbow.

The arrangement of the colours was that of the ordinary rainbow, the yellows and yellow-reds being best marked.

The appearance of the sun, as seen in St Andrew Square, was hazy, with light wisps of clouds and large patches or blurs of mist in that part of the sky, giving rise to ill-defined shadows, so that I looked about to see if there were any appearance of halos or mock-suns visible, but none were seen.

By Dr Ferguson.

Dr Ferguson stated that he had read in the *Inverness Advertiser* that a similar phenomenon had been seen at Nairn and neighbourhood on the same day from eight in the morning till midday. It is described as a pale blue arch on a white ground, having the outline and position of a rainbow, but differing from it in its remarkable fixity.

I saw the rainbow at Edinburgh from the middle of the east division of Queen Street. I think it was at eleven, but the friend in whose company I was at the time says it was twelve. I looked at the bow attentively, but not critically, as I was not aware of the exceptional character of the phenomenon. The day was fine, but there was a haze in the sky, which gave an indistinct outline to the masses of cloud which occupied the northern heaven. It looked to me like a cloud rainbow, as its continuity seemed to correspond to that of the cloudy mass on which it was seen. I remember particularly one spot to the west of the middle, where there was a partial break in the clouds and a similar defect in the bow. As regards colour, it wore the appearance of a bleached rainbow, with an indefinite stratification of tints. I did not specially mark each gradation of colour from the red to the blue, but I had a distinct impression of such. The colours were faint, and the perception of the coloured rainbow effect was as much, or more, due to contrast, than to the colours taken individually.

7. Extract of Letter to the President from H. E. Rosevelt, Esq., dated New York, Dec. 23, 1877.

"I saw the 'Phonograph' the other day, and though it is very crude I was much interested. I would briefly describe the idea as follows:—

"To the centre of an iron diaphragm is attached a metallic point resting against a strip of paper or tinfoil. You speak against diaphragm through a mouthpiece, at same time the paper being drawn under the points. The vibration of diaphragm and point indent the paper to various depths, making an undulating (as it were) mark, &c. Now, if said paper is afterwards drawn under the point, the diaphragm vibrates exactly as your voice made it vibrate, and all the sounds are reproduced exactly as you said them, making a most astonishing effect—singing, laughing, and articulate words were all reproduced. Of course it is by no means perfect, but it is very interesting. By attaching a point to one of our strong telephones we could record any messages sent. Though the phonograph is purely mechanical in its ordinary use, I mention all this to you, as you will probably soon hear of it, and naturally would not believe it a possible thing. The above is the invention of Mr Eddison, who showed it to me himself."

Monday, 4th February 1878.

Sir C. WYVILLE THOMSON, Vice-President, in the Chair.

In delivering the Neill Medal to Dr Traquair, the Chairman made the following remarks:—

Dr Ramsay Traquair,—The Neill prize for the triennial period 1874–77 has been awarded to you by the Council of this Society, nominally for your paper on the structures and affinities of *Tristichopterus alatus* (Egerton), communicated to this Society within the required period, and published in the 27th volume of the Transactions of the Society. The Council have, however, also taken into account, as they are entitled to do by the conditions of the prize, the many contributions which you have made during some years past to the knowledge of the structure of recent and fossil fishes.

I may mention as among the more important of these a memoir "On the Asymmetry of the Pleuronectidæ, as elucidated by an examination of the skeleton in the Turbot, Halibut, and Plaice," published in the Transactions of the Linnean Society of London for the year 1865; a paper "On the Cranial Osteology of Polypterus," in the *Journal of Anatomy and Physiology* for the year 1870; and a work on "The Ganoid Fishes of the British Carboniferous Formations," now in course of publication by the Palæontographical Society.

You have been fortunately placed during the last few years in a locality remarkably rich in the group of fossils to which you have paid special attention, and already three parts of a valuable series of papers "On new and little-known Fossil Fishes from the Edinburgh District" have appeared in the Proceedings of this Society.

I have much pleasure in presenting to you the Neill medal in the name of the Council of the Society.

The following Communications were read:—

1. Chapters on the Mineralogy of Scotland. Chapter III. The Garnets. By Professor Heddle.

In this third chapter Dr Heddle submits the results of his analyses of garnets from thirteen localities.

Notwithstanding the great frequency of the occurrence of the mineral, it is generally so largely contaminated with quartz that he was unable to procure, from many localities, specimens fit for examination.

Three varieties of garnet new to Scotland—one of these being altogether new—have, however, been the reward of the present investigation.

On a hill lying north of Balmoral,—Creag Mohr,—two of the new varieties were found, both in limestone.

The first, the rarer, was the *water garnet*, or colourless garnet; he second was the *grossular*, or gooseberry garnet, hitherto found only in perfection on the Wilni river in Siberia.

The *cinnamonstone* of Glen Gairn has next been analysed. This is finer in colour than the Ceylon mineral, but so flawed as to be useless for purposes of jewellery.

Not so the *pyrope* of Elie—the "Elie rubies," as they used to be termed. Dr Heddle regards these as, weight for weight, the most valuable of Scottish gems.

Analyses of *common garnet* from Yell in Shetland, Killiecrankie and Meall Luaidh in Perthshire, Knockhill in Banff, and Clach-an-Eoin in Sutherland follow.

There are, lastly, analyses of a *new* garnet from five localities—four of these in Ross-shire; the other, Ben Resipol, in Argyll.

This is a *precious garnet*, containing about fifteen per cent. of oxide of manganese. Its formula places it intermediate between the ordinary precious garnet of Bohemia and the manganesian garnet of America.

This garnet, in all the localities where it is found, is of a fine currant-red colour, due probably to the manganese. From Resipol and the Raven's Rock, near Strathpeffer, pieces large enough for cutting might probably be obtained.

The lime garnets occur in certain of the limestone beds, at the upper waters of the Don and Dee, and not in others; and Dr

Heddle thinks it probable that this fact will aid in the tracing out of the individual beds—far from an easy matter in that troubled district.

The conclusion of the chapter is devoted to a speculation upon the metamorphism of these limestones.

2. On the Strength of the Currents required to Work a Telephone. By Professor Tait.

(*Deferred from January 7th.*)

Perhaps the most singular fact connected with the telephone is the excessive feebleness of the currents which suffice to work it. I have had no opportunity of testing any but rough arrangements set up by present or former students of my own, so that I cannot judge how far my results may apply to the instrument as sold.

1. A striking illustration of the feebleness of the currents required is furnished by using a Holtz machine driven *very* slowly, without condenser, and with its terminals so close that the discharge is barely audible, and certainly invisible except in the dark. When insulated wires were led from these terminals to the telephone (placed in a distant room) the effect was very curious. The instrument gave a hissing sound, quite comparable in intensity with that which was produced directly when the terminals of the machine were widely separated, one connected with the ground and the other with a large conductor discharging by brushes into the air, the machine being turned rapidly. The telephone continued to give audible sounds with slow turning, even when the terminals of the machine (somewhat tarnished) were *pressed into contact.*

2. To measure roughly the intensity of the current, I placed one prong of an unmagnetised tuning-fork about half an inch in front of the sending telephone, and measured by a microscope and scale the extent of its vibrations when the note just ceased to be audible to a listener at the receiving telephone. Next I substituted for the receiving telephone an exceedingly delicate astatic galvanometer, with very small moment of inertia, and measured the swing produced by one definitely assigned motion of the prong of the tuning-fork. By means of a known thermo-electric couple, I determined the strength of the current corresponding to the observed

swing. The result is, of course, only a very rough approximation. It is that a single Grove's cell would produce, in a circuit of somewhere about a billion B.A. units resistance, a current sufficient, if reversed 500 times per second, to produce an audible sound in the telephone I employed.

3. Several attempts at explanation of the action of the telephone have been given here and elsewhere, and others are promised for to-night. For my own part, I think there are at least *three* separate causes at work in the telephones I have used.

There can be no doubt that the inventor's own explanation is, at least to a certain extent, correct. For we can easily dispense with the magnet in the receiving telephone, using merely a thin iron disc in front of a coil. And Mr Blyth has, I believe, found that we may make the disc, even in this case, of copper, and yet have transmission (though very feeble) of intelligible sounds.

But this cannot be the full explanation. For it does not attempt to account for the peculiar nasality of the transmitted speech. Without going more closely into the matter, the difference of quality between an open and a closed pipe suggests a certain amount of constraint as the cause. And we know that the sounds in the original telephone of Reiss were produced by molecular motions due to magnetism in soft iron. Mr Blyth has shown conclusively that molecular motion in the magnet itself has a large share in the results, because he has successfully substituted other metals than iron, and even non-conductors, for the disc, and in certain cases finds that he can dispense with the disc altogether.

Besides this, however, it seems to me that there is a third cause, which in certain cases is more effective than either of the others. This is suggested by the fact that (at least with the instruments I have tried) high notes, even of comparatively small intensity, are much more clearly transmitted than low notes,—indicating that the *rapidity* of the molecular change has a great deal to do with the result. In fact, in this respect, the telephone is really a variety of the so-called *curb-key*, giving very sudden reversals.

These considerations have led me to fancy that *rapid* change of form in matter, whether paramagnetic or not, may probably be capable of detection by the telephone, for the associated electric currents may be in certain cases powerful enough to produce audible sounds. I am at present engaged in a series of preliminary experiments on this subject.

3. Experiments with the Telephone. By James Blyth. Communicated by Professor Tait.

In the telephones used in these experiments the permanent magnets were of the ordinary horse-shoe form, about 4 inches long. No cores of soft iron were attached to the poles, the insulated wire, No. 26, being wound directly round both, in such a way that a current circulating through it followed the direction of Ampère's currents. The vibrating disc was the bottom of a shallow can of thin tinned iron, $2\frac{1}{2}$ inches in diameter, supported directly above the poles of the magnet, and almost touching them. The receiving instrument was so arranged that any kind of disc could be easily substituted for the ordinary vibrating plate, and tested by sound from the same transmitting instrument, so as to allow of a comparison being made between discs of various materials. Having first ascertained that no sound was audible when no vibrating plate was used, I tried discs of the following substances, and have arranged them approximately in the order of distinctness with which the sound was heard:—

Ferrotype plate.
Thin steel.
Thin iron.
Wire gauze (fine).
Do. (a little coarser).
Cast-iron plate, $\frac{3}{4}$ inch thick.
Sheet copper.
Sheet brass.
Sheet zinc.
Tinfoil.
Vulcanite.
Thin fir wood.
Paper.
Vulcanised India-rubber.
Cast-iron, 6 inches thick.
Slice of raw potato.
Slice of fresh butter.

With the view of testing what effect would be produced by varying the position of the wire coil on the leg of the magnet, I constructed a telephone, so that the coil could be easily slipped up and down the leg, while the sound was being sent from the transmitting instrument. Very little difference in the sound was observable till the wire coil was brought near the neutral point of the magnet. It then became very faint. I next slipped the wire coil entirely off the magnet, and simply held it touching the pole side-ways. The sounds were still quite audible, and continued to be so when the coil was removed an inch or more away from the magnet. This led me to try the effect of dispensing altogether with the steel magnet, and merely holding the helix of insulated wire opposite the centre of the vibrating plate. With an iron disc a very faint sound was heard, but it became more distinct when a disc of copper was used. From this it would seem, that the vibrations producing the sound are caused by the attraction between the currents in the helix and the induced currents in the copper disc.

Still using the same transmitting instrument, I next employed a receiver, in which a soft iron core, carrying the insulated wire, was rigidly attached to the vibrating plate. This was accomplished by rivetting the head end of a screw-nail into its centre, and winding the wire round it. The pole of a steel magnet was then fixed, just clear of actual contact, opposite the head of the nail, and, with this arrangement, sounds were distinctly audible, though not so loud as when the wire coil surrounded the magnet.

With the view of rendering the telephone an instrument for detecting the existence of very feeble currents, I constructed a pair with magnets similar to those already described, but with ferrotype discs. These were joined together by a strong semicircular spring in such a way that they could be put on, after the manner of spectacles, and stick close to the sides of the head, inclosing both ears. India-rubber rings were provided for the double purpose of excluding all extraneous sound, and avoiding any disagreeable pressure on the ear.

With this arrangement, I proceeded to test for the existence of thermo-electric currents. A copper and iron junction was inserted in the circuit of the telephone, and attached to a spring in such a way that it could be made to vibrate rapidly out and into a gas flame. A distinct grating noise was heard in the telephone. A very peculiar rasping sound was produced when the ends of a copper and iron wire were rubbed together while hot, and also when one of the wires was attached to a file, and the hot end of the other drawn rapidly along it.

4. On the Theory of the Telephone. By Prof. George Forbes.

The Telephone, invented by Mr Graham Bell, is an instrument by means of which any sounds, musical notes or spoken sentences, sounded into an instrument at the sending end of a telegraphic wire may be reproduced at the receiving end. The theory of the Telephone is two-fold. First, the mechanical theory of the nature of sounds and of speech; and secondly, the theory of the action of the instrument. The first part is well known. All sounds consist of a succession of waves propagated through the air, the rate and intensity of their succession determining the nature of the sound, in pitch, loudness, and tone. If the same succession of waves as a speaker makes in using his voice can be reproduced in the air in contact with the ear of any other person, by any means whatever, the latter person will hear a fac-simile of the sound uttered by the former. A theory of the action of the Telephone is distinctly given by the inventor in the specification of his patent. The receiving and sending instruments are identical. Round the end of a bar magnet a coil of fine wire is wound, connected with a telegraph wire on the one hand and the earth on the other. In front of these a thin iron plate is caused to vibrate by the sounds uttered in its neighbourhood. Consequently, it approaches and recedes from the pole of the magnet. This alters the magnetism in the neighbourhood. In Faraday's language, 'the lines of force are altered in position, so as to go across the coil of wire.' In consequence of this, by the well-known laws of electromagnetic action, a current of electricity is sent in one direction with each approach of the vibrating plate, and in the opposite direction with each recession. This succession of currents reaches the receiving end with a strength proportioned to the extent of vibration of the iron plate. Here they circulate through the coil, and so intensify or diminish the magnetism of the bar. Thus, the vibrating plate at the receiving end is more or less attracted, according to the direction of the current. Hence, the plate at the receiving end vibrates to and fro in vibrations which are synchronus and proportionate to those at the sending end; and so the sounds are reproduced.

This theory gives a *vera causa* for the action of the instrument in its normal form, and probably explains *in part* the true action which takes place, but the author believes that this is not the whole of the action. The fact that the inventor was working upon this theory when he designed the instrument, does not prove the theory to be correct, as is shown by his having at first employed a powerful battery with the instrument, believing it to be necessary, and it was only after diminishing the number of cells without impairing the action of the Telephone that it occurred to him to try it without a battery at all, when he found the action to be equally good.

The author's mistrust of the theory commenced when he learnt that a thick iron plate might be used, whose vibrations must be extremely minute. And when he learnt that the instrument might be used with plates of glass, wood, tinfoil, or vulcanite, or even without any plate at all, he was convinced that the theory was imperfect, and resolved to propose another theory which seemed more consistent with facts.

This theory is divided into two parts—the action at the sending end, and that at the receiving end. It is very simple, and is founded upon two well-known experiments.

The theory of the action at the sending end is founded upon an experiment by Sir William Thomson, who finds that iron subject to magnetic induction has its magnetism increased when slightly stretched in the direction of its magnetization, and we may assume that it is diminished when compressed. Now, what happens in the Telephone? Take the simplest case which has been tried, *i.e.*, where there is no vibrating plate. Sound waves (which are capable of passing through solid bodies as they do through the air) strike the end of the bar magnet, and are propagated through it. Waves of alternate compression and extension pass through the length of the magnet. Consequently, the magnet is alternately increased and diminished in intensity. Hence, currents are generated in the coil of wire, and transmitted through the telegraph wire alternately, in opposite directions, as in the theory hitherto adopted. The presence of a vibrating plate of any kind may help this action, by increasing the intensity of the waves transmitted through the bar magnet; and if the plate be of iron it will undoubtedly help to propagate these currents.

According to the author's theory, the action at the receiving end is explained by an experiment ascribed to Page. If a bar of iron be surrounded by a coil of wire connected with a battery, a chink is heard when the connection is broken, and a feebler one when the connection is re-made. If the connection be made, and broken rapidly by attaching one end of the wire to a file and drawing the other along the file, a series of chinks are heard succeeding each other at the same intervals as the contacts are made and broken. It is due to the lengthening of the bar on magnetisation and the rearrangement of the molecules of the iron. It takes place also when the iron remains partially magnetised. In this case successive contacts increase the magnetism of the bar. In the Telephone successive currents pass in opposite directions through the coil. Hence the bar is lengthened and shortened alternately, and chinks are made at intervals corresponding to the intervals between successive waves at the sending end. Hence the sound waves at the sending end are reproduced at the receiving end alike in rapidity and in intensity. When a vibrating plate of any kind is used it increases the intensity of the sound, and if it be of iron the attraction will increase the effect.

Since a chink is given off both when the magnetism is increased and diminished it might be expected that the note should be heard an octave higher. But since the one is more intense than the other, the combined effect should be to give two notes, a loud one in the tone of the speaker, and a feeble one an octave higher. This might account for the common remark that a piano sounds *wiry* through a Telephone, and that a clear lady's voice is *squeaky*. But it is not certain that this effect is produced, for the chink is produced at least in part by the lengthening and shortening of the magnet. Either of these alone will give a chink, but when following each other in rapid succession they compress and rarefy the air in contact, in periods exactly agreeing with the compressions and rarefactions originated at the sending end.

In confirmation of the supposition that the sound is produced by successive *chinks* of the material of the magnet, it was stated that in a very large and massive pair of Telephones made by Mr Wm. Bottomley, jun., when any one is speaking through one of them the sound is distinctly (though not articulately) heard by every one in the room where the other is placed, and there is no doubt that the sound seems to come *from the magnet itself.* The same is done in the case of a compound Telephone with 25 bar magnets to one iron plate exhibited by the author.

In conclusion, the author considers that the theory now explained must be taken to supplement that of Mr Graham Bell (1), because the phenomena alluded to undoubtedly take place, and (2), because it is the only theory which explains the action when the vibrating plate is not made of iron, or when there is no vibrating plate at all.

5. Some Experiments with the Telephone.

By John G. M'Kendrick, M.D.

During the past two months my attention has been directed to the telephone, chiefly as an instrument which illustrates in a remarkable way the delicacy of hearing. As some of these experiments are novel, and may possibly assist those who are investigating the physical phenomena of the instrument, I beg to give a short account of them to the Society. To save circumlocution in description, the term "proximal" will be applied to the telephone receiving the stimulus, and the term "distal" to the one at the other end of the arrangement. Thus, if A talks to B, A will use the proximal, and B the distal telephone.

1. *Transmission of the Sounds of Tuning-Forks.*—At an early stage of my investigations, finding it convenient to have a constant source of sound at the proximal telephone, I placed a telephone opposite one of the limbs of a tuning-fork, kept in vibration by an electro-magnet, the current being interrupted automatically by the fork itself. At the distal telephone the sound could be distinctly heard. Then I removed the disk from the proximal telephone, and allowed the limb of the fork to play in front of the naked end of the core. The distal telephone then sounded more distinctly than before. The next step was to remove the metal disk from the distal telephone, and apply disks made of vulcanite, porcelain, glass, paper, wood, a large piece of iron $1\frac{1}{4}$ inches thick, a thick rod of iron, 8 inches in length by 1 inch in diameter. Still the sound of the proximal telephone could be heard, though faintly. There could be no doubt of the fact. To ascertain whether the effect might not be due to the communication of mechanical vibrations

from the tuning-fork beside the proximal telephone, I found, that moving the latter away 3 or 4 inches from the vibrating fork, was followed by silence of the distal telephone; indeed, to cause the latter to sound, the proximal telephone has to be placed as near the fork as possible.

The following experiment is of interest, as showing the effect of a thin metallic disk in connection with the distal telephone:—Remove the disk, and apply in its place a plate of glass; listen, and the sound of the proximal telephone will be heard faintly. Then put one of the metal disks on the glass, and on listening again, the sound will be found much intensified. It may be still further intensified by placing a second disk on the surface of the first, taking care to have a few morsels of paper interposed so as to prevent them from touching.

The next step was to ascertain whether or not sounds could be heard at the distal telephone without the use of a disk there. There can be no doubt that sounds may be heard in these circumstances. They are very faint, however, and have not the same quality as those heard with a disk.

The arrangements at the distal telephone were further modified in the following experiment:—The disk was removed from the distal telephone; and the base of a glass cylinder, 8 inches in length by 2½ in diameter, filled with water, and closed at each end by a ground glass cover, was placed against the core. Very feeble sounds from the proximal telephone were heard, when the ear was placed at the other end of the glass cylinder. These sounds were slightly intensified by slipping in a thin metal plate between the base of the cylinder and the core; but when a second disk was placed between the ear and the other end of the cylinder, the sounds were nearly as loud as with the ordinary telephone. In this experiment, the two disks were separated by a distance of 8 inches, and it is difficult to explain the phenomena as depending merely on conduction of sound.

At this stage it occurred to me to place opposite to the core of the distal telephone a tuning-fork of the same pitch as that opposite the proximal telephone. The result was, that the one tuning-fork set the other in movement, so that the sound of the distal tuning-fork could be heard throughout the room. As is now known, the same observation was made by Dr Rontgen, and recorded by him in "Nature. It was interesting as being a proof of the actual vibration of the metallic substance at the distal tuning fork.

2. *Transmission of the Sounds of Organ Pipes.*—If an organ pipe sounding loudly be brought into close proximity with a proximal telephone, its sound is heard distinctly at the distal telephone. If then a pipe of the same pitch be placed opposite the distal telephone, and it be sounded feebly, the sound is reinforced when the pipe at the proximal telephone is also sounding; but I have never succeeded in causing one pipe actually to initiate sound in the other. Mr Aiken of Falkirk, however, has informed me by letter that he has observed this phenomenon. I found also that there was no intensifying effect when either of the disks was removed.

3. *Transmission of the Sounds of Vibrating Strings.*—I succeeded in causing a string to vibrate by means of the telephone as follows:—Two catgut strings, each having a telephonic disk cemented to the centre of the string, were tuned as accurately as possible to a pitch of about C; one disk was placed opposite a large and powerful telephone (the proximal), made by Mr William Bottomely, and the other (the distal), opposite a similar instrument; on plucking the string at the proximal telephone, the string opposite the distal also sounded, though feebly. This experiment holds out the hope, that by suitable arrangements the sounds of strings might be transmitted for musical purposes.

4. *Optical Observations of the Movements of the Disk.*—There can be no doubt (1) that sounds may be heard even when the disk is firmly pressed against the ear, so as to prevent its vibrations to a great degree; and (2) that sounds may be heard even without any disk on the distal telephone. A disk, or moving metallic body capable of magnetic action, is always necessary at the proximal telephone. On the other hand, the existence of a disk, free to vibrate, at the distal telephone always intensifies the sound, and, in particular, it appears to be almost essential for distinct articulation of language. One would therefore infer that the disk must move as a whole. These movements I have studied in various ways, and they will still be under observation.

1*st method.*—A strongly vibrating tuning fork was kept going opposite the proximal telephone; a fine glass rod or filament, 3 inches in length, was cemented to the centre of the disk of the distal telephone; to the other end of this rod two common microscopical covering glasses, having between them a drop of blood, were also cemented so as to be in a horizontal position. The drop of blood thus fixed to the telephone disk was then brought under a Hartnack's $\frac{1}{4}$th inch, magnifying about 300 diameters, and the coloured blood corpuscles were brought into accurate focus. A key was placed near the distal telephone, by which the current might be transmitted or interrupted at pleasure. On opening the key, the circular-coloured corpuscles at once assumed an oval form, and were put somewhat out of focus, plainly the result of lateral movement. Thus, by an application of Lissajoux's method of observing vibrations optically, the movements of the disk could be seen. I found that the amplitude of the movements of the vibrating fork (moving 60 vibrations per second), near the proximal telephone, was about $\frac{1}{6}$th of an inch; the movements of the corpuscles were about half their diameter, or about $\frac{1}{6500}$th of an inch. On stopping the current by means of the key, the movements almost immediately ceased. Again, on taking away the glass rod from the disk, and applying the ear to the disk, the low booming sound of the fork could be heard. It is, therefore, evident that these sounds were produced by movements of the disk, the amplitudes of which were about the $\frac{1}{6000}$th of an inch, an interesting example of the sensitiveness of the ear. The minimum limit of excitation of the ear has been thus stated:—The faintest sound perceptible is that caused by a ball of pith, 1 milligramme in weight, falling 1 millimetre in height upon a glass plate, may be heard at a distance of 91 millimetres from the ear (*Schafhäutl*).

The telephone appears to be an instrument which illustrates the extreme sensitiveness of the ear, even better than the method of Schafhäutl just alluded to. The length of the hair cells in the cochlea (which are stated to be tolerably stiff and rigid) is about the $\frac{1}{8000}$th of an inch. One of them could therefore perform an excursion of the $\frac{1}{4000}$th of an inch. If, as Helmholtz states, the mechanical arrangements of the bones of the ear are such as to diminish to one-third the excursion of the membrana tympani, and if no further reduction took place in the internal ear itself, the oscillations of the plate I observed, and which were capable of causing a sensation of sound, would only amount, when they reached the hair cells, to about the $\frac{1}{18000}$ of an inch, and thus cause a movement of the auditory hair through about $\frac{1}{4}$th of its possible amplitude of excursion.

2*d Method.*—The movements may also be observed by throwing obliquely a beam of light on a reflecting surface on the disk, and catching the reflected ray on a screen. I did not find this method so satisfactory as the one just described.

3*d Method.*—I cemented the disk of the distal telephone to the thin membrane of one of Koenig's manometric capsules, then there was no difficulty in observing the oscillations of the flame in a rapidly revolving mirror, when a strongly vibrating tuning-fork was placed opposite the proximal telephone. I failed, however, in seeing any movements produced by speech. The method, however, is capable of more refined application than the means at present at my disposal will allow.

4*th Method.*—I have attempted to record the movements of the disk graphically by attaching to it a lever bent at right angles at one end, bringing the point on a rapidly moving surface. No oscillations could be detected. Such oscillations were, however, recorded when, instead of a disk, I used forks at the proximal and distal ends, as already described. The movements of the distal fork were then recorded, but it was observed that the sound of the fork was audible long after all oscillations could be recorded, showing that the movements of the fork were too delicate to be recorded by this method.

5. *A Mode of Intensifying the Sound of the Telephone.*—In studying the transmission of sounds of various kinds, I had occasion to use a rapidly revolving wheel opposite the proximal telephone. The wheel in my possession, which moves with greatest velocity is one about 4 inches in diameter, having placed transversely on its circumference twenty-four rectangular bars of iron. The wheel is driven by two electro-magnets, and the current employed was obtained from twenty of Sir William Thomson's tray cells. With this power it performs about eighty revolutions per second. When the proximal telephone was brought near the circumference of the wheel, the distal telephone sounded so loudly that a rattling sound like that

caused by the wheel could be heard quite distinctly all over the room. Evidently the effect of bringing the horizontal iron bars on the circumference of the wheel rapidly in front of the core of the proximal telephone was to intensify the currents of the telephones. The currents were so strong that a disk held in front of the core of the distal telephone could be felt vibrating by the hand.

The following Gentlemen were duly elected Fellows of the Society:—

James Alfred Ewing, 22 India Street.
Rev. John Wilson, M.A., Bannockburn Academy.
Robert Macfie Thorburn, Uddevalla, Sweden.
Andrew Peebles Aitken, Sc.D., 16 Gillespie Crescent.
John Milne, Mechanician, Trinity Grove, Edinburgh.

Monday, 18th February 1878.

Sir WILLIAM THOMSON, President, in the Chair.

The following communications were read:—

1. The application of the Graphic Method to the determination of the efficiency of a direct-acting Steam-Engine. By Professor Fleeming Jenkin.

2. On the Disruptive Discharge of Electricity. By Alexander Macfarlane, M.A., B.Sc. Communicated by Professor Tait.

(*Abstract.*)

Last summer session, with the assistance of Messrs Salvesen, Connor, and Stewart, I applied the method of measuring great differences of potential, described in a paper by Mr Paton and myself (Proc. Feb. 19, 1877), to investigate the laws of passage of the electric spark. The method essentially consists in connecting the prime conductor of the Holtz machine, not with the electrometer directly, but with an insulated spherical ball. This ball acts inductively upon another insulated spherical ball which is in connection with the electrometer.

Before proceeding with the investigation proper, I tested the accuracy of the method by applying it to determine how the induced potential of the ball in connection with the electrometer depends on the distance between the centres of the balls. I found that the equation

$$V = 6081\ r^{-1} - 42{\cdot}26,$$

where V denotes the induced potential, and r the distance, between the centres of the balls, satisfies all the observed values of V for values of r greater than twenty-four centimetres, but for smaller values of r the function requires to be corrected by being multiplied by

$$f(r) = {\cdot}524 + {\cdot}02\ r.$$

Our method, when applied to measure the difference of potential required to pass a spark through air at the atmospheric pressure between parallel metal plates at different distances, gave a result agreeing well with that which Sir William Thomson discovered to be true for small distances. The function for V, the difference of potential in terms of s, the length of the spark is

$$V = 66{\cdot}94\ \sqrt{\{s^2 + {\cdot}205\ s\}}$$

the equation of an hyperbole, whose semi-transverse axis is ·1025 centimetres, and semi-conjugate axis 6·8623 centimetre-gramme-second units. We observed, for lengths of spark, up to 1·2 centimetre.

From the above equation we infer that—

$$R = 66{\cdot}94 \sqrt{\left\{ 1 + {\cdot}205\ \frac{1}{s} \right\}}$$

where R denotes the electrostatic force; from which it is evident that as s becomes smaller, R becomes greater. But when the discs were heated well, immediately before the taking of the observations, the curve obtained satisfies the equation—

$$V = 87{\cdot}04\ s - 19{\cdot}56\ s^2$$

a parabola; from which we deduce

$$R = 87{\cdot}04 - 19{\cdot}56\ s.$$

This result, in my opinion, establishes the truth of Clark-Maxwell's hypothesis, that the greater electromotive force required at the smaller distances is due to the existence of condensed gas on the surface of the discs. Precisely similar results were obtained when hydrogen was substituted for air.

The method, when applied to measure the difference of potential required to produce a ·5 centimetre spark at different pressures of the air, shows that for the range between the atmospheric pressure and twenty mm.,

$$V = \cdot 0458 \sqrt{\{p^2. + 203\,p\}}$$

where p denotes the pressure in millimetres of mercury.

The electric strengths of several gases were determined by comparing the differences of potential required to pass a ·5 centimetre spark through the gas at 746 mm. pressure.

Dielectric.	Electric Strength.
Air,	1
Carbonic acid,	·95
Oxygen,	·93
Hydrogen,	·63
Coal gas,	·93

Several series of observations of the difference of potential required to produce a spark between spherical surfaces for distances up to 15 centimetres confirm the result published in the paper already referred to, that the difference of potential is proportional to the square root of the length of the spark.

3. On the Compressibility of Water, Sea-Water, a four per cent. Chloride of Sodium Solution, Mercury, and Glass.

By J. Y. Buchanan, M.A., F.R.S.E.

4. On the Action of Heat on some Salts of Trimethyl-Sulphine.

By Professor Crum Brown and J. Adrian Blaikie, Esq.

(*Abstract.*)

The authors describe the preparation, properties, and action of heat upon the following compounds of trimethyl-sulphine:—

1st, The sulphide $[(CH_3)_3S]_2S$;
2d, The hyposulphite (thiosulphate) $[(CH_3)_3S]_2S_2O_2, H_2O$;
3d, The oxalate $[(CH_3)_3S]_2C_2O_4, H_2O$.

1st, The sulphide was prepared by dividing a strong solution of trimethyl sulphine hydrate ($[(CH_3)_3S]HO$) into two equal parts, saturating one with sulphuretted hydrogen, and then adding the other. The strong aqueous solution thus obtained was placed under a bell jar filled with coal-gas over anhydrous phosphoric acid. After a certain concentration had been attained, sulphide of methyl began to evaporate along with the water. When a solution prepared in this way was sealed up in a glass tube, a very slight rise of temperature caused the liquid to separate into two layers, the upper consisting of sulphide of methyl and the lower of aqueous solution—

$$[(CH_3)_3S]_2S = 3(CH_3)_2S.$$

This aqueous solution has all the characters of an alkaline sulphide. It dissolves sulphur, forming an orange-coloured polysulphide, it dissolves sulphide of antimony, gives the characteristic reaction with nitro-prusside, and, when treated with an acid, gives off sulphuretted hydrogen, a salt of trimethyl-sulphine being left in solution. When exposed to the air, the sulphide is rapidly oxidised, hyposulphite being produced.*

2d, The hyposulphite is best obtained by oxidation of the polysulphide, by exposure to the air.

It crystallises in clear four-sided prisms with one molecule of water of crystallisation. Analysis gave the following results:—

	I.	II.	III.	Calculated.
C, . .	24·85	24·64	...	25·35
H, . .	6·94	7·06	...	7·04
S, . .	...	...	44·65	45·07

The salt is very hygroscopic, sparingly soluble in alcohol, and gives all the reactions of an alkaline hyposulphite. Over anhydrous phosphoric acid it loses 6·37 per cent. of water—the formula requires 6·33 per cent.

The anhydrous hyposulphite, when carefully heated to about 135° C., gives off sulphide of methyl—5·545 grammes, heated in this way, lost 1·308 grammes of sulphide of methyl, equal to 23·58 per cent., and left a white crystalline substance, soluble in water, alcohol, and ether. The authors are at present engaged in the investigation of this product.

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* See *ante*, pp. 320, 321.

3d, The oxalate is obtained by treating the iodide with oxalate of silver. It crystallises, with one molecule of water, in clear hygroscopic plates.

Analysis gave the following results :—

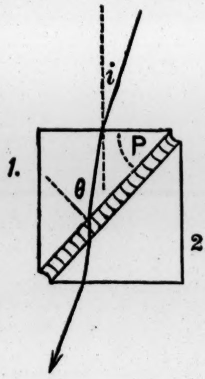
	I.	II.	Calculated.
C, ...	36·95	36·78	36·92
H, ...	7·87	7·89	7·69

On carefully heating the salt to 110° C., the water of crystallisation is given off. At 146° C. the anhydrous salt decomposes into sulphide of methyl and pure oxalate of methyl—

$$[(CH_3)_3S]_2C_2O_4 = 2(CH_3)_2S + (CH_3)_2C_2O_4.$$

The chromate and the iodate of trimethyl-sulphine were also prepared. Heated to about 140° C. they both fuse, and almost immediately explode.

5. Extracts from two Letters by Professor Quincke on the Refractive Indexes of Glass and Quartz, as tested by Reflection from the Surface. Communicated by Sir William Thomson.



In answer to your question about the alteration of surface in quartz crystals, I place the glass or quartz plate whose refractive index χ is to be determined between two right-angled flint-glass prisms, with oil of cassia, and measure the angle θ, at which total reflection begins from the hypothenuse of the first flint-glass prism; the angle θ can easily be calculated from i, μ the refractive index of the flint-glass, and P the angle of the prism. Sunlight falls from a collimator with a slit upon the system of flint-glass prisms, and after passing through the second prism is examined by a direct vision set of prisms, I turn the system of flint-glass prisms until the spectrum appears to be broken off at a definite Fraunhofer line, measure the angle i, and thus obtain—

$$\operatorname{Sin} i_{,} = \frac{\sin i}{\mu}, \ \theta = \mathrm{P} - i, \ \chi = \mu \sin\theta.$$

Of course one can use flint-glass prisms other than right-angled ones, and, in fact, for measures of quartz, I have had flint-glass prisms made by Steinheil in Munich, in which i was only a few degrees. In a plate of quartz cut perpendicularly to the optic axis, one can easily determine in this way the refractive indices of the ordinary and extraordinary ray by interposing in front of the eye a Nichol's prism in the proper position. In fresh quartz plates I obtained almost exactly the same values as Rudberg:—

Rudberg.

B	C	D	E	F	G
1·54090	1·54181	1·54418	1·54711	1·54965	1·55425
1·54990	1·55085	1·55328	1·55631	1·55894	1·56365

Quincke ǂ *zur Axe.*

1·54108	1·54207	1·54412	1·54710	1·54966	1·55365
1·54987	1·55065	1·55338	1·55622	1·55892	1·57166

Quincke Quartz ⊥ *zur Axe* ↺

1·54022	1·54092	1·54318	1·54575	1·54845	1·55246
1·54880	1·54955	1·55245	1·55533	1·55801	1·56163

Quincke Quartz ⊥ *zur Axe* ↻

1·53958	1·54087	1·54335	1·54649	1·54868	1·55243
1·54789	1·54933	1·55199	1·55508	1·55758	1·56192

The differences of the individual measurements in different specimens I attribute to difference in the properties of the specimens of quartz themselves.

In old quartz surfaces which have already been about twenty years in my possession, and in others which I have found in the physical collections in Wurtzburg and Heidelberg, the refractive index for the Fraunhofer line D varied between 1·5141 and 1·5374 for the ordinary ray, and between 1·5216 and 1·5470 for the extraordinary.

I allow the first surface of the quartz to adhere by capillary attraction, and my large horizontal circle, by which μ and i are measured, reads to two seconds.

Crown glass plates from Steinheil, which had lain ten or twelve years in a press, and whose refractive index for D was 1·5245, gave, by total reflection at the surface, the refractive index 1·4903.

The alteration of the surface appears to me to be due, in quartz as in glass, to a chemical change of surface, perhaps to the vapour in the air forming a hydrate of silicic acid, or a hydrate of silic.

The above measures and arrangements have not yet been published, but they are entirely at your service if you can use them in your article on Elasticity.

I hoped to have sent you with the former ones some measures of the refractive indices of natural quartz surfaces, but then the observations have to be made with reflected light, which impairs their accuracy; besides, in spite of great trouble, I have not been able to procure any quartz whose surfaces were flat enough and complete enough for this inquiry. The only crystalline surface which I could examine seemed to have the same refractive indices as fresh polished surfaces. Besides, I am not astonished to find different refractive indices in different quartz crystals, since I have invariably found slight variations in the optical constants even for light transmitted through different specimens of crystals, for instance, in the amount of rotation of the plane of polarisation. Even if one had kept the crystal for thousands of years under the same physical conditions, for instance, at the same temperature, &c., still, according to my opinion, the mode in which it was originally formed would affect its final stationary condition. Crystals, like human beings, and like films of liquid upon heterogeneous solid or liquid surfaces, carry with them during their whole existence the mark of their origin or birth. Two bodies can only show properties extremely alike, never exactly the same.

Professor Jenkin called attention to experiments made by Mr Gott in St Pierre, and published in the Journal of the Society of Telegraph Engineers. Mr Gott converted two siphon recorders into a telephonic system by mechanically connecting the suspended coils with diaphragms. In this experiment the only conceivable mode of action was analogous to that suggested by Professor Graham Bell as the explanation of his telephone. This explanation, if not complete, was not, in Professor Jenkin's opinion, erroneous. Professor Jenkin announced that he had, with Mr J. A. Ewing's assistance, constructed one of Mr Edison's phonographs, and that this instrument, like the telephone, gave a nasal intonation to the words spoken by it.

Monday, 4th March 1878.

D. MILNE HOME, LL.D., Vice-President, in the Chair.

The following Communications were read:—

1. Proposed Theory of the Progressive Movement of Barometric Depressions or Storms; being in continuation of the Paper read before the Society on July 5, 1875. By Mr Robert Tennent.

In this paper it is not proposed to discuss in their relations to storms the effects of rain, of the earth's rotation, of areas of high and low pressure external to the storm-area, and of the prevailing westerly winds, which are doubtless occasional factors in the progressive movement of storms. What it is intended to show here is that storms possess in themselves a self-motive power, by which their onward movement over the earth's surface is determined.

It will tend to clearness if attention be pointed at the outset to two very different kinds of barometric depressions. The one which accompanies the true cyclones of the tropics is, on comparison with the height of the disturbance, of very limited extent, while the other, of which our European storms are examples, and which alone are dealt with in this paper, is of very great extent as compared with the height. The modes in which these two distinct kinds of barometric depressions tend to fill up are widely different from each other, owing particularly to the degree in which the element of friction is introduced by the forces set in motion within the storm itself.

Mode in which a Barometrical Depression of a Narrow Diameter fills up.—In this case, since the diameter of the depression is small as compared with the vertical height of the storm, it follows that the inflow of the air-currents towards the area of lower pressure will take place over a surface of comparatively small extent, and will consequently meet with but little retardation from friction. Hence the process of inflow toward the centre of the barometrical depression will take place with comparative facility in true tropical cyclones of small extent, and be therefore characterised by steep barometrical gradients.

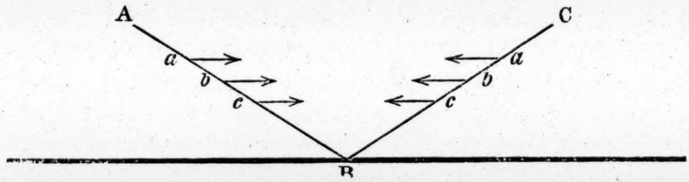


Fig. 1.

Let A B C (fig. 1) represent a vertical section of such a depression, and let the small arrows *a*, *b*, *c*, show the direction in which it will tend to fill up over a surface of comparatively small extent, accompanied by a steepening of the gradients B A and B C.

Mode in which a Barometrical Depression of Wide Diameter fills up and in the process tends to open out.—In European and American storms, the barometer at their centres falls about as low as in tropical cyclones, but the barometric gradients are much less steep, and the disturbances cover a much more extensive region. Redfield has pointed out that their vertical height is often not more than the two-hundredth part of their horizontal breadth. It follows from this essential difference between these storms and true cyclones that the mode of inflow of the air-currents towards the central low pressure can neither take place in the same manner nor with a similar facility, but, on the contrary, much friction must now be called into play, owing to the extensive resisting surface over which the air-currents are drawn.* Since friction is much greater near the earth's surface than it is aloft, it follows that such storms are characterised by greatly retarded surface currents, and rapid upper currents. Looking at the mass of the atmosphere on the outskirts and outside the area of the storm as the source whence the incurring air-currents draw their supply, it is obvious that as regards the true cyclone the source of supply is easy and copious, whereas in the case of our widespread European storms, supply is comparatively scarce, and therefore defective. In truth, as the lower and denser air-currents are, owing to the enormous extent of surface they cover, much retarded by friction, the main source of supply for the inflowing currents is chiefly to be found in the rarer, more mobile, and more rapid upper currents which flow comparatively free and unimpeded. This essential difference in the mode of inflow of the air-currents of these two types of storms is considered to be due to differences in the amount of friction, accompanied also by a great difference in the introduction of the important element of Time. The difference in velocity between the surface and upper currents is often very great. Thus Glaisher has shown from his balloon ascents that the upper currents are sometimes five or six times more rapid than the surface currents, while from observations made at the top and base of Mount Washington, it was shown that on one occasion the wind on the summit was blowing with a velocity of ninety miles an hour at the time that a calm prevailed at the base of the mountain.

The following illustration will show what is meant in this paper by the expression opening out all round. Let a barometric depression be formed several hundred miles in diameter, and let the stratum of air resting on the surface be calm, whilst aloft upper currents flow in upon the centre at the rate of ninety miles an hour. In this case the central inflow will be carried out entirely by the upper currents, and consequently there will result from this mode of inflow an outward extension or an opening outwards of the area of diminished pressure.

In fig. 2, A B C may be supposed to represent the vertical section of the barometric minimum of a true tropical cyclone as in fig. 1. Let now D B F represent a vertical section of a barometric minimum of a very much larger diameter. When it opens up all round in the way described by means of rapid upper currents, it will be accompanied by a lowering of the surrounding gradients from the upper part of which the main source of supply is derived. The result is that the depression originally embracing a circular space whose diameter was D F will widen out, and the diameter extend to G H. The inflow which in this way takes place along the gradients bounded by the circle whose diameter is D F, as shown by the arrows *a* and *f*, will now have the effect of lowering the gradients B F and B D to B H and B G. The result of this may be represented by the removal of the air comprised in the spaces G B D and H B F.

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* "Proceedings of R.S.E." vol. viii. p. 613 and p. 614.

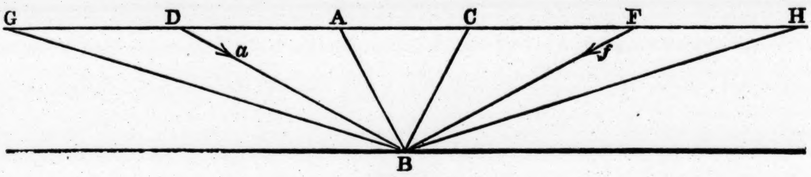

Fig. 2.

When the air moves more rapidly aloft than it does near the surface, it may be conceived as moving onwards, not in vertical, but in inclined columns; and we have endeavoured to show* that this mode of inflow is attended by "lifting," and to some extent by fictitious pressure, by which is meant that although the barometer indicates correctly the elasticity of the air, still it no longer represents its real mass overhead, but a pressure more or less diminished owing to the mechanical movement of the air and to friction. The opening out or extension of the area of barometric minimum, with D F for its diameter, to a wider area, having G H for its diameter, is effected by the upper currents, indicated by *a* and *f* of fig. 2. These upper currents, which flow in upon the low central depression from a great distance all round, have their source of supply in the upper still atmosphere around and outside the circular space over which they blow. It is here where outward extension and shallowing out commences, viz., along the curve which indicates the points where removal of air first begins to exceed restoration. This line is what has been previously designated as "the curve of outward propagation."*

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* "Proceedings," vol. ix. p. 412.

This point, which is considered to be of great importance, may be better understood by an illustration. If a river flowing down an incline does so uniformly, and at an equal rate of speed, removal will equal restoration; but if in the lower part of its course a more rapid removal is inaugurated, while restoration or supply above remains as before, the curve representing the point at which the increased removal begins to travel upwards will represent the forward movement of this curve of outward propagation or extension.† Let A B C (fig. 3) represent the inclined surface of a river, A B the lower part of its course, in which a more rapid flow, and consequently a more rapid removal, has commenced, and D B the curved line which represents increased removal beginning to travel upward. Let us suppose that A is the low centre of a large sheet of water, towards which currents set in all round, and are there carried off. If now the inflow towards this centre there begins to increase in speed, the curve which extends all round will be propagated outwards, or in a direction opposite to that in which the currents flow. A depression may thus practically fill up by shallowing out, extending all round, and thereby lowering the gradients. When a depression fills up rapidly, it is, of course, attended by a correspondingly rapid rise of the barometer; but when it shallows out by a process of lateral extension the barometric rise is much slower. It is only in the case of an imperfect fluid, such as air, flowing over a resisting surface that the more special modes of inflow here insisted on can occur. On a frictionless surface they could not take place. When we consider the differences in the high pressures surrounding the depression on its different sides, and the different qualities of the air, as regards moisture and temperature, in different parts of the

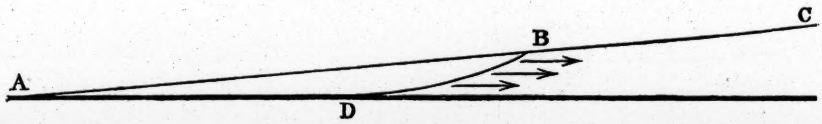

Fig. 3.

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* "Proceedings," vol. viii. p. 613.

† *Ibid.* vol. viii. p. 614.

depression, it is evident that the mode of inflow cannot be uniform all round, and that horizontal extension will take place in some one particular direction. This direction will be determined by the curve of outward propagation, which thus marks the direction of the onward course of the depression or storm.

The Mode in which Opening Out takes place, accompanied by a transference of the Depression from West to East.—If a vessel be lowered into the sea a corresponding amount of water will be displaced. Let it then be supposed to move forward to the extent of its own length, an opening out ahead will take place, and a filling up astern. The original displacement will thus be closed up, but a similar displacement will be found in the new position to which the vessel has moved. Owing to its rigidity the vessel moves forward unaltered, but as the surrounding water is mobile, it cannot do so, hence it opens out and fills up. The currents which run aft on each side of the vessel represent its forward movement, while there is no real movement of the surrounding water, except its gradual transference to the rear, in the direction opposite to that in which the vessel moves. When an area of low pressure moves in an easterly direction over the British islands, and to a distance equal to that of its own diameter, it will do so in a somewhat similar manner, viz., by opening out and filling up. There is, however, an important difference to be noted; for while the tranference of the water, which enables the vessel to move forward, takes place on each side in the direction of the stern, the transference of the air, when the depression moves eastwards, takes place on the north segment from the front, where it opens out to the rear where it fills up, as all observation shows. It may be pointed out here that while depressions move forward, the aerial particles, or the mass of air, does not move forward, and viewed in this light the curve of outward propagation may be regarded as being a *purely ideal curve.*

The severe N.E. storms, which are of so frequent occurrence as to form a marked feature of the climate of the New England States, were long ago pointed out to advance on this region in an opposite direction from that in which they blow. Let the area of the storm be represented by the circle A B C F (fig. 4), then the front part A B C is the curve of outward propagation, which moves toward the N.E. in the direction of the arrow D E, this being the contrary direction to that in which in the segment A B the N.E. wind blows. Clouds apparently moving from the S.W. are often observed in this part of the storm, where N.E. wind first begins to exhibit itself. No general movement of the air, however, takes place, because it is here that it first commences and continues to be generated from the still atmosphere ahead, where the curve of outward propagation advances upon it. This continued generation also represents what takes place in the clouds. Their apparent movement, which is due solely to their additional formation, is the result of atmospheric changes which must here take place. Hence occasionally they do not represent the force and direction of the wind. Similarly seamen often observe a gale approaching from some point to leeward.

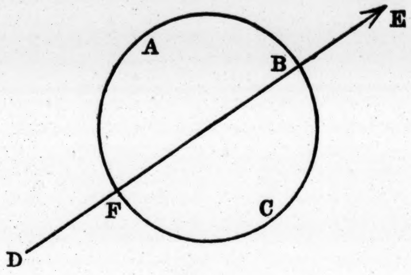

Fig. 4.

It has been supposed by some writers that the progress of storms is due to the area of low pressure being impelled forward by the high pressure usually found near their rear, aided by the diminished pressure in front, which is due to heat and vapour. If this were the case, it is evident that a derangement would take place as regards the circular symmetry of the storm and the direction of the spirally inblowing winds, owing to the enormous friction which these widespread storms would encounter in being pushed forward over the earth's surface. The more evident of these derangements would be the steepening of the barometic gradients in front of the storm, a result the reverse of what observation shows to take place. The Rev. Clement Ley has shown that the isobars are widest, and gradient consequently lowest in the front of the storm.* Further, if this were the cause of the progress of storms, areas of depression would move more rapidly over the ocean than over the land. Professor Loomis has shown, however, that while the average rate of progress of depressions over the ocean is 19 miles an hour, over the land the rate is 26 miles an hour. It is also evident that if a depression moves forward accompanied by spirally inflowing winds,—considered simply as inflowing air-currents, and not as representing an efficient motive force capable of generating forward movement,—a derangement as regards their force and direction would take place, especially on those occasions when the onward march of the storm attains a speed of 70 miles an hour. But in no case has such a derangement been observed.

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* See "Journal of Scottish Meteorological Society," vol. iv. p. 149.

Proposed Theory as to the Progress of Storms.—When, however, the depression is not being impelled forward in the mode above described, but in virtue of an opening out in front, and when the air-currents, inflowing spirally upon the centre, are regarded as an effective motive force capable of causing progressive motion, no derangement can be supposed to take place as regards the force and direction of the air-currents; and no steepening of the gradients in front will take place, since the change in the position of the depression is effected with facility by the transference of the air from the front to the rear along the north segment of the depression.

A depression advances by means of these spirally inflowing currents, owing to the difference which exists in their *mode of inflow*, and consequently in their force and direction; and it is in this way that outward extension in one direction is carried out. This difference in the mode of inflow is mainly due to the introduction in a much higher degree of the element of friction into the currents of the front segment; in this way scarcity of supply is produced in this part of the depression, and the effect is intensified by the ascending tendency of the air in this segment of the depression. On the other hand, the currents which enter in at the rear are much less retarded by friction, because there is a descending tendency of the air in this segment, and, consequently, supply is copious.* The low central pressure, by travelling onwards towards the region in front, where supply is scarce, and where opening out takes place, practically overcomes this scarcity of supply, and thus uniformity of inflow is practically restored by progressive movement. If there was no progress, and if the depression was accompanied by this want of uniformity in the mode of inflow, it would then tend to fill up. Progress is therefore the means by which a depression is maintained.

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* "Proceedings," vol. viii. p. 613–615.

Since the difference in the mode of inflow is due to a difference in the amount of friction called into play, we have here, probably, the reason why storms advance more rapidly over the land than over the ocean. The deflection of the winds produced by the earth's rotation, and the irregularity in the amount of pressure at different points immediately outside the area of the depression itself, have doubtless important bearings on this whole question.

It is very generally supposed that the spiral inflow to the centre is supposed to be there carried off by means of a vast ascending current from the centre. This opinion is, however, open to serious objections, but which it would be out of place here to discuss.

The general result is, that progress is regarded as being due to an irregularity in the direction and force of the spirally inflowing currents, arising from the different degrees in which the element of friction is called into play in the different segments of the storm, accompanied or aided by high pressure on the right of the direction in which the depression moves.

Summary.—When a depression is formed with a narrow diameter, it fills up rapidly if it has a copious supply from the surrounding atmosphere, and the filling up is of course attended by a rapid rise of the low central barometer. But if the depression has a very large diameter, the element of friction must enter largely into the problem. The retarding influence due to this cause will necessarily be greatest in the denser currents near the earth's surface; while the light, rarefied, and mobile upper currents will move with great facility. Hence the main source of supply will be thrown on these freely flowing upper currents. The rapidity and facility with which they inflow give rise to horizontal extension or shallowing out aloft, and this extension would take place equally all round if the inflow were uniform in all the segments of the depression. If, however, this uniformity of inflow does not exist, but is more copious in one segment and scarce in another, then extension and shallowing out will take place towards that particular direction where supply is scarce. Hence progressive movement takes place in this direction, and is attended by filling up in the rear. When supply is equally copious in all directions, the low central barometer rises rapidly; but when the supply is not copious and uniform all round, the barometer does not rise, because, though the depression fills up in the rear, there is a corresponding opening out in the front, and the central barometer remains low —thus producing the central spiral inflow which generates the motive force to which the progressive movement is due.

2. On the Thermo-electric Properties of Charcoal and certain Alloys, with a supplementary Thermo-electric Diagram. By C. G. Knott, B.Sc., and J. G. MacGregor, D.Sc. Communicated by Professor Tait.

3. On the Auriferous Quartz of Wanlockhead. By Dr Lauder Lindsay.

4. On the Discharge of Electricity through Turpentine. By A. Macfarlane, B.Sc., and Mr R. J. S. Simpson. Communicated by Professor Tait.

5. Remarks on the Phonograph. By Professor Fleeming Jenkin and Mr J. A. Ewing.

Professor Fleeming Jenkin and Mr J. A. Ewing exhibited a phonograph which they had constructed, and also some curves drawn on paper representing the indentions produced in the tinfoil of the instrument by vowel sounds.

The phonograph exhibited had been constructed from a description of the instrument invented by Mr Edison, and consisted of a barrel about four inches diameter and four inches long, mounted on a spindle on which a square-threaded screw had been cut. One bearing of the spindle was cylindrical, and the other was a nut in which the screw worked. A fly-wheel handle turned the spindle and barrel, which advanced during each turn by a distance equal to the pitch of the screw. A helical groove, about $\frac{1}{20}$ in. in breadth, was cut on the surface of the barrel, having the same axial pitch as the screw on the spindle.

A smooth strip of tinfoil was gummed round the barrel, and indentations produced in this strip by a point attached to a vibrating disc stretched across a short brass cylinder $2\frac{1}{2}$ in. diameter.

The same vibrating disc was used to produce the indentations and to reproduce the sounds.

The usual ferrotype plate had not been tried as a vibrator, for hard metal discs were found to give a disagreeable resonance to the voice and its reproduction. A slack tinfoil disc had been used with good results; the marker was attached firmly to a small disc of stiff paper $\frac{3}{4}$ in. diameter, gummed to the tinfoil; a short piece of watch spring was also attached to the marker, so as to give the disc a rapid period of vibration.

A sentence reproduced by this disc was loud enough to be heard by many people standing round, and sentences had been heard by several persons who understood them without any previous idea of what they were. This result could not, however, always be secured. When the sentence was known to the hearers, it appeared to be given back with startling accuracy. The vowel sounds were more distinct than the consonants, but the consonants also were distinctly to be heard. The tinfoil vibrator gave more articulate sounds when slack and irregular than when it was neatly strained over the end of the tube.

An oil-silk disc had also been tried with no spring, and a simple marker attached to a disc of mica, gummed to the oil-silk. This disc gave purer sounds than the tinfoil, but they were not nearly so loud. The indentations on the tinfoil were excellent to the eye, and quite as large as with the tinfoil.

The oil-silk answered best when irregularly stretched.

An india-rubber film, with a similar mica disc and rigidly attached marker, had also been tried, and gave beautiful records on the tinfoil strip, but this disc failed to reproduce the sounds accurately, having a note of its own.

This latter disc was used to produce records which were subsequently enlarged and shown on paper in the form of curves. The india-rubber was preferred for this purpose, because the gentle and uniform pressure which it gave did not tend to obliterate the records. The curves exhibited showed the form of the indentations magnified about five hundred times. This magnification was effected by two compound levers, of which the second was a glass siphon like that of Sir William Thomson's recorder. This siphon deposited the trace of the curves on a paper strip rolled on the periphery of the large disc used by us for the experiments on friction published in the "Transactions of the Royal Society of London" for 1877.

The authors had not yet had time to analyse these curves and compare them with Helmholtz's theory of vowel sounds.

The advantage of recording the curves corresponding to the several sounds indirectly by means of the tinfoil, instead of directly, as in the case of the phonautograph, consists in the fact that with this arrangement the amplitude of the oscillations may be greatly magnified without introducing any sensible error due to the inertia of the moving parts. The paper band and tinfoil record were moved at a very slow speed, and their relative advance was rendered quite definite by a mechanical connection between the two barrels on which they were wound.

The following Gentlemen were duly elected Fellows of the Society:—

Charles Davidson Bell, Retired Surveyor-General, Cape of Good Hope, 19 Dean Terrace.
James Blyth, M.A., 8 Middleby Street.
R. K. Galloway, M.A., Jesus' College, Cambridge, 10 West Claremont Street.

Monday, 18th March 1878.

Sir WILLIAM THOMSON, President, in the Chair.

The following communications were read:—

1. On Thermal Conductivity. By Professor Tait.

(*Abstract.*)

This paper contains the results of a laborious series of experiments and calculations carried out during the last ten years, the object being the repetition and extension to other metals than iron of Forbes' experiments, described to the Society in 1862 and 1865.

As a check upon his work, the author first experimented upon the iron bar employed by Forbes, and reproduced, almost exactly, Forbes' results for that metal—the most notable feature of which, besides the first really trustworthy determination of absolute conductivity, was the discovery that the thermal conductivity of iron is diminished by heating, and is very nearly proportional to the reciprocal of the absolute temperature.

None of the other metals tried by the author, viz., copper (two specimens, selected as being specially good and specially bad conductors of electricity), lead, and German silver, gave at all analogous results. Their absolute conductivities have been determined; and the changes produced by heat in these conductivities were, where any, very slight, and uniformly in the direction of improved conductivity at higher temperatures.

A second part of the paper is promised, to contain comparisons of thermal and electric conductivity in the same substance, with an investigation of the "Thomson effect."

2. On the Wave Forms of Articulate Sounds. By Professor Fleeming Jenkin and Mr J. A. Ewing.

In this paper the authors gave a preliminary account of experiments made with the help of the phonograph exhibited at the last meeting. The following results have been obtained:—

1. The vowel sounds can be produced by maintaining the relative pitch of the simple tones of which they are composed constant, although the absolute pitch of those simple tones may vary greatly. The authors found that whether they turned the barrel of the phonograph at a greater speed or at a less speed than that which had been maintained while the human voice produced the indentations, the several vowels were given back with equal distinctness.

This experiment proves that within certain limits a given wave form in the tinfoil produces a given vowel independently of the rapidity with which it passes the pricker of the vibrating disc. It proves therefore that within these limits a given group of partials is competent to produce a given vowel effect, whatever be the absolute pitch of the prime tone, and of the relative partials. This result is of much importance, since Willis, Wheatstone, Donders, and Helmholtz seem all to consider that a certain absolute pitch is characteristic of each vowel.

[NOTE.—*May* 24.—Since the above communication was made to the Society, the authors have improved their phonograph, and also their apparatus for enlarging the record embossed on the tinfoil. For this reason they have substituted in the figures shown better curves than those which were originally presented to the Society. Subsequent experiments with the improved instrument also render it desirable to add a few words to the statement made in the text as to the effect of altered speed in the quality of the sounds given by the phonograph. The original experiment consisted in speaking the set of vowel sounds, A, E, I, O, U (pronounced in Italian fashion), to the phonograph, and then listening to the sound given when the instrument was run at various rates. The vowels were observed to maintain their relative places, so as to form the complete series in which each individual sound was by contrast at least recognisable as the vowel which had been spoken. This experiment has since been frequently repeated with perfect success. Again, if a single vowel sound, such as "*oh*," be sung to the phonograph at an ordinarily high pitch, it will be found to remain *oh* through a considerable range of speeds; but the *oh* whose pitch has been raised by quickening the phonograph seems to the authors brighter than the *oh* sung by the human voice at a correspondingly high pitch; that is to say, it resembles "*awe*" to some extent. Similarly the spoken "*awe*," when accelerated, passes into a sound like *ah*. The change appears to be greater with some vowels than with others, and also to depend partly on the pitch at which the vowel has been originally sung. The instrument is, however, even in its present improved form, not well adapted for testing any fine gradation of the quality of sound. Although the authors now feel unable to speak with any confidence as to change of quality produced by a change of speed, they consider it proved that the vowels, A, E, I, O, U, remain distinguishable by contrast when all their constituents are simultaneously raised or lowered to a considerable extent, but they do not consider that the phonograph is as yet sufficiently perfect to enable them to judge how far the definition of constant quality suggested in § 3 below would or could be accepted by the ear.]

The authors' experiments do not prove that a given vowel always produces a definite wave form.

2. A given person pronouncing or singing a vowel sound at a constant pitch produces a wave form of remarkable constancy. This proves that the relative phases of the simple tones, of which the compound tone is built up, do not alter, while a single vowel is being sung or intoned on one note. The same person or different persons pronouncing the same vowel on different occasions, will frequently reproduce wave forms closely similar, but the experiments are not yet sufficiently extended to determine whether a given voice speaking the same vowel at a given pitch will always produce the same wave form.

3. A given person singing the same vowel throughout an octave, often produces wave forms of different appearance on different notes, although he may be endeavouring to keep the quality of his vowel constant. It must on this be remarked that up to the present time there has been no standard or criterion as to constancy of quality at different pitches. This has been a mere matter of appreciation by different ears. A standard wave might, indeed, be taken and declared to be the form which gives a constant quality at all pitches. The phonograph is sufficiently distinct in its utterance to allow it to be said that such a standard *a* or *o* will be accepted within a considerable range as the same vowel, but it cannot be affirmed that all hearers would say that the quality of the vowel was constant in their judgment at all pitches. It is, however, possible that henceforth constancy of quality may be defined as that given by constancy of wave form.

4. The vowels are heard equally well, or nearly so, whether the machine is run backwards or forwards. The authors put in the saving clause "nearly" everywhere, because the sounds obtained from the phonograph are not sufficiently clear to warrant more absolute conclusions. This reversibility is a necessary consequence of Helmholtz's theory that the ear decomposes the compound tones into their harmonic constituents, and is indifferent to the phase relation of these.

5. Not only the vowels but the consonants are substantially the same when produced backwards. This result is novel to the authors, and is, they believe, quite unforeseen. There is little to wonder at

in the fact that *r*, or *s*, or *θ*, which are continuous, should sound alike backwards and forwards, but it is very remarkable that sounds like *p* or *k* at the end of the syllable should, when produced backwards, give the same effect as *p* and *k* at the beginning of a word.

The authors tested this singular fact in the following way:—They first arranged that a simple combination, such as "abafa" or "afaba," should be said to the instrument while one observer was out of the room. He then came in, and heard the word backwards. If the original word had been "abafa," he was in all cases able to say that he heard "afaba"; if the original word had been "adaka," he heard "akada." Inasmuch as he was quite uncertain whether the word spoken to the instrument was "adaka" or "akada," this experiment was in itself almost decisive. All the letters of the alphabet were tried in this way. Fearing, however, that when the alternative lay between two consonants, these might be recognised by some other quality than their true sound, the authors next proceeded to test the fact by speaking short words, such as "amma," "abba," and so forth, to the instrument. These were recognised by an observer from outside when spoken backwards. "Alla" was recognised at once; "appa" was called "abba"; "amma" was recognised at once; "atta" was called "adda."

The greatest difficulty was met with in distinguishing *s* from *f*, but the difficulty was not greater backwards than forwards. It seems due to the fact that the instrument used will not record the high tones which are obviously present in these sounds.

When the fact had been established that a consonantcould be sounded backwards between two vowels, monosyllables were next tested: "ba" was reversed and heard as "ab;" "ka" was reversed, and heard as "ak."

This is even more remarkable than the previous results, inasmuch as the mode of pronouncing *b* or *k* at the end of a word is different from that employed when it begins a word.

Finally, by pronouncing any word backwards, such as "noshaeesossa," and then reversing the instrument, the original word (association) was clearly heard. With a little practice, a sentence might be spoken backwards, and then heard intelligibly forwards.

The letter *h* was not in the phonograph to be distinguished from *ch*, and gave a single reversible sound. "Ho," when turned back, sounded distinctly "och."

Since both vowels and consonants sound alike backwards and forwards, it appears that articulate speech may be divided into a series of successive parts, each having a character of its own, and each reversible so far as sensation is concerned.

The separate reversible sensations correspond in the main to the letters of our alphabet, but our letter *i* represents at least two reversible elements, and *ch*, *ng*, or *th*, on the contrary, are single sounds. Clear, clean sounds, as spoken by educated people in all countries, seem remarkable for the small number of the elements that enter into each word. An uneducated man, speaking a dialect or *patois*, will make each nominal vowel consist of a large group of simple reversible sounds running into one another.

6. The wave form for the letter *r* has been recorded. It agrees with that obtained by Donders on the phonautograph, and consists of a series of simple curves, similar to harmonic curves, which gradually increase to a maximum, and then gradually decrease to a minimum. The number of waves separating the maximum from the minimum intensity of the sound was in the examples observed 7 or 8. The length of the whole period was therefore 14 or 16.

7. The wave forms obtained from the letter *o* in figs. 4 and 5 illustrate the difficulty which the ear, even of trained musicians, feels in determining the octave in which a given note is sung. It will be seen that each period consists of two very nearly equal parts, and that the amplitude of the second partial has obviously been large as compared with that of the prime tone. To one ear it might seem natural to call the pitch that of the second partial, since this was the loudest tone; to another the pitch would be that of the prime, because this tone was lowest in pitch. Naming a given note by the lowest of its component tones, irrespective of the relative intensity of the partials, seems to be a convenient but conventional rule.

8. The instrument as at present made does not record all sounds. This is shown by the fact that its reproduction of the French *u* is very imperfect, and *ee* is only sometimes well heard from it.

The curves laid before the Society were obtained by multiplying the traces on the tinfoil some 300 times by means of a system of levers. The lines were drawn on the paper by the plan of frictionless marking invented by Sir William Thomson, and used in his Siphon Recorder. A rise in the curve (on the page) corresponds to a hollow in the tinfoil. The arrow pointing from right to left, indicates the direction in which the tinfoil passed under the vibrating pointer when the sounds were uttered.

The authors hope soon to be able to communicate to the Society the results obtained by subjecting the experimentally drawn wave forms to harmonic analysis.

Fig. 1 gives the curve made by the sound of *oo*, as in "food," on the note *b*; fig. 2 is *oo* on *f′* by the same voice; and fig. 3 is a still higher *oo* by another voice. As the speed at which the phonograph was turned was very nearly the same in all the examples, the relative pitches are approximately given by the lengths of the periods.

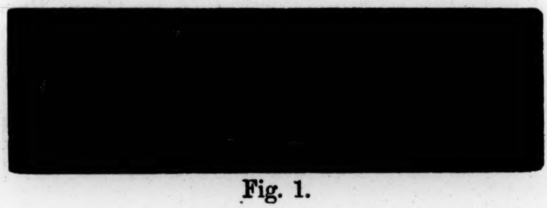


Fig. 1.

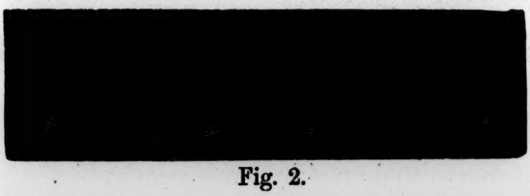


Fig. 2.

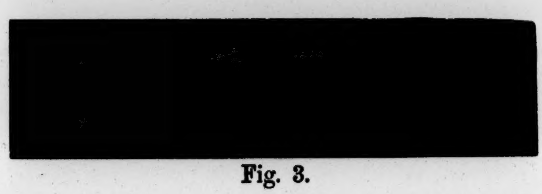


Fig. 3.

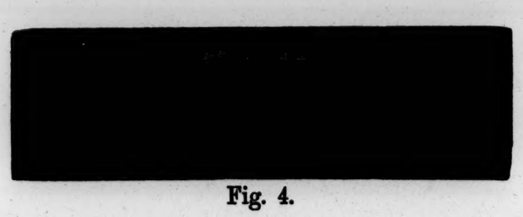


Fig. 4.

Figs. 4, 5, and 6 show the waves given by the sound "*oh*," spoken at various pitches by the same voice (that of Dr Crum Brown). Figs. 7 and 8 are given by the sound *a* in "father," the first on *c* by Sir R. Christison, the second on *c′* by Professor Jenkin.

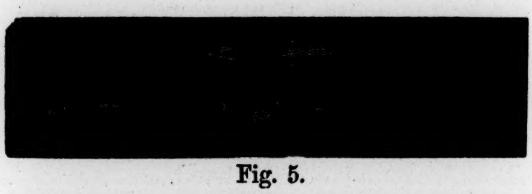


Fig. 5.

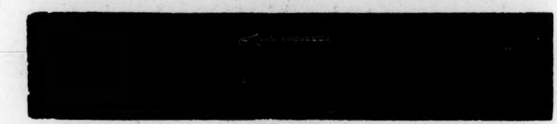


Fig. 6.

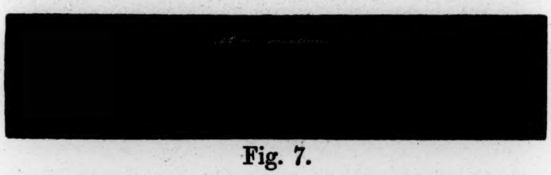


Fig. 7.

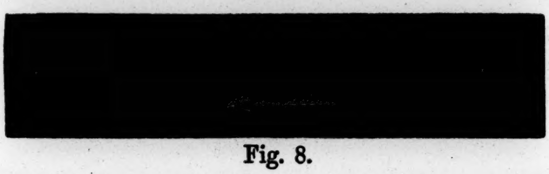
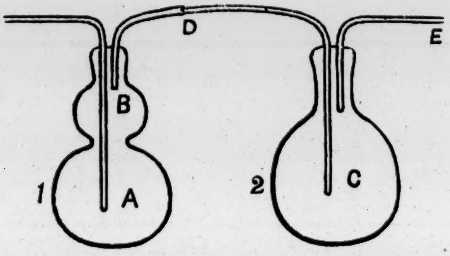


Fig. 8.

3. On the Action of the Chlorides of Iodine upon Acetylene and Ethylene. By George M'Gowan.

Before giving the action of ethylene on the chlorides of iodine, I may shortly describe the method of preparing the latter, which I found to be the most advantageous:—



A weighed quantity of perfectly dry iodine being introduced into bulb A of flask 1, perfectly dry chlorine gas is passed over it, A being at the same time heated over a water-bath to about 60° to 80° C. The ICl_3, as it is formed, volatilises and condenses almost entirely in B, only a small portion escaping into flask 2. Chlorine is passed until no more iodine remains in bulb A. If the heating be omitted, only the outermost layers of iodine are converted into the trichloride, a kernel remaining unacted on. (Compare *Christomanos*, Berl. Ber. X. No. 5, p. 434.)

The tubes connecting the two flasks are ground into one another at D, so that no cork or caoutchouc fittings are required. This is absolutely necessary.

By attaching a small $CaCl_2$ tube to E, no moisture can enter the apparatus. The flasks can be weighed either together or separately. When passing ethylene or acetylene over either of the iodine chlorides, I found it best to cleanse and dry flask 2, leaving the gas to act on the chloride in 1. In that way any volatilised matter from 1 was condensed in 2, and the loss of weight was very trifling.

By adding the required weight of iodine to the ICl_3, and heating gently over a water-bath (the vessel being corked), ICl is easily prepared. I invariably got it in the state of long needles.*

Action of Ethylene (C_2H_4) *on* ICl_3.

By passing pure dry C_2H_4 over pure dry ICl_3 at 0° C., the two compounds *chloride* and *chloriodide* of ethylene are formed.

This reaction takes place quantitatively, thus :—

$$(C_2H_4)_2 + ICl_3 = C_2H_4Cl_2 + C_2H_4ClI.$$

	grms.	grms.
Weight of flasks, ...	169·2	
„ $+ ICl_3$, ...	202·5 =	33·3 ICl_3
„ after saturation with C_2H_4 .	210·1 =	7·6 C_2H_4

Theory requires 7·98 grms. C_2H_4.

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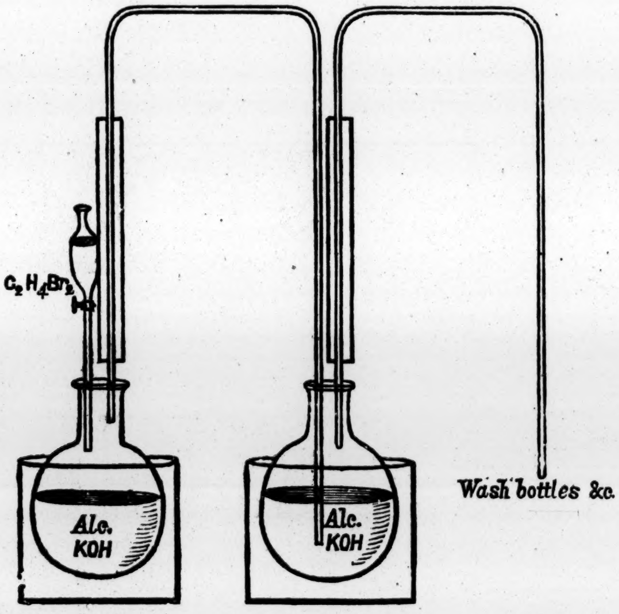
* It is generally stated that ICl dissolves in alcohol and ether, forming yellow solution. (See Watts's *Dict. of Chem.* vol. iii. p. 293, new ed.) The fact is, that if ether (perfectly free from water and alcohol) be dropped into ICl, a most violent reaction ensues. If, on the other hand, ICl be dropped into a large excess of ether (or alcohol) at 0°, it dissolves, at first forming a pale yellow-brown solution ; but, in the course of a few minutes, the ether becomes darker and darker from separation of iodine. The chlorine probably acts on the ether to form dichlor-ether ($C_4H_8Cl_2O$) for the most part. This requires to be investigated.

After saturation, the fluid was almost colourless, having only a pink tinge of iodine. The two above fluids can (after washing with dilute KOH and drying) with some difficulty be fractionated.

$C_2H_4Cl_2$ boils at 85° (Fittig).
C_2H_4ClI „ 147° (Maxwell Simpson).

Preparation of Acetylene (C_2H_2) *from Ethylene Dibromide* ($C_2H_4Br_2$).

Having prepared about a kilo. of $C_2H_4Br_2$, I decomposed it according to the method described by Sabanjeff (*Annalen*, Band 178). Briefly described, this method is as follows :—Into a thick flask of about one litre capacity, more than half filled with a concentrated alcoholic solution of KOH, and heated over a water-bath to 100° or thereabouts, $C_2H_4Br_2$, pure or diluted with alcohol, is slowly and regularly dropped from a separating funnel. The C_2H_2 disengaged, passes off through an upright Liebig's condenser, also affixed to the flask, by which means most of the monobrom-ethylene (C_2H_3Br) formed at first, and carried away along with the C_2H_2, is returned to the flask. Some of it, however, escapes undecomposed from this flask into a second one, also containing alcoholic KOH, and fitted with



an upright condenser, precisely in the same manner as the first. In this second flask it is practically entirely decomposed. The C_2H_2 is then passed through alcohol at 0° (to absorb traces of C_2H_3Br), then through two Wollf's bottles filled with water, and lastly through two $CaCl_2$ tubes. The gas may then be considered practically pure. To test this, I passed it for about half an hour through the two weighed flasks, surrounded by a mixture of ice and salt, but at the end of that time they had not gained in weight to speak of.

To prepare first cuproso vinyl oxide—

$C_4Cu_4H_2O$ (*Berthelot.*)

and then decompose this by dilute hydrochloric acid, is an extremely tedious and troublesome operation, and offers very little corresponding advantage. Sabanjeff discusses the other known methods for the preparation of C_2H_2 very fully in his paper. I also tried some of them, but found the above by far the best of all.

Action of Acetylene (C_2H_2) *on* ICl_3.

On passing C_2H_2 over pure dry ICl_3 at 0°, combination takes place with great evolution of heat. I always found more or less iodine separated out. My chief object being to try to prepare the compound, C_2H_2ClI, I did not investigate farther the products formed. Probably, if the ICl_3 were kept cold in a mixture of ice and salt, and the C_2H_2 diluted with air and passed over it very slowly, one might get the compound $C_2H_2Cl_3I$.

Action of C_2H_2 *on* ICl.

(*a.*) On pure ICl at 0°.

The evolution of heat produced by the combination was so great that more or less iodine always separated out.

(*b.*) On ICl dissolved in excess of ether at 0°.

The ether used must be perfectly free from alcohol and water. The C_2H_2 was passed as quickly as possible through this ethereal solution, to reduce as much as might be the secondary reaction of the ICl on the ether. It was absorbed greedily.

After saturation the ethereal solution was diluted with water, when a heavy liquid settled to the bottom. This was washed with dilute NaOH solution, and then with water, and dried over $CaCl_2$.

What ether remained in the above liquid was distilled off at 35° to 40° in a current of C_2H_2. Even at this temperature a slight decomposition occurred (iodine separating), so I stopped heating. It was found practicable, however, to fractionate the liquid (with very slight decomposition) in a current of steam. Repeated fractionating yielded

(1.) Ether + $C_2H_2Cl_2$, &c. (not examined).
(2.) C_2H_2ClI +.
(3.) Solid $C_2H_2I_2$.

To ascertain the composition of the above liquid I analysed it.

+ 0·6687 grm. ignited with pure CaO gave—

Tube . . .	22·9350 grms.
„ + (AgCl + AgI)	24·2482 „
„ + (AgCl + AgCl)	22·9207 „

(after converting AgI into AgCl by current of Cl).

AgCl (on filter) . . 0·0127 grm.

Calculated, this gives—

	Found.	Theory for C_2H_2ClI.
I	67·38 . .	68·77
Cl	18·83 . .	17·59

The above figures do not agree very exactly, but I had too small a quantity of the compound to be able to fractionate it perfectly. It was found impossible to determine its boiling point, as it decomposed very rapidly on heating, with separation of iodine.*

Spec. Grav. . . . 2·41 at 13·5° C.

Acetylene chlor-iodide is an ethereal liquid with much the same odour as the corresponding ethylene compound. Its vapour attacks the eyes. It does not decompose to any extent if kept in the dark.

Remarks on Acetylene Iodide ($C_2H_2I_2$).

I prepared from 50 to 60 grms. of the above compound by passing the gas through a saturated alcoholic solution of iodine. As the two combine very slowly, the iodine must be distributed in numerous small flasks, which ought to be hung on a glass rod, horizontally placed, to admit of their being shaken from time to time. (See Sabanjeff, *Annalen*, Band 178; also Semenoff (for $C_2H_4I_2$), *Zeitschrift für Chemie*, 1864.)

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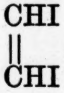
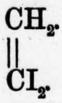
* It seemed to boil at first, on heating very rapidly, between 180° and 190° C., but this is not reliable.

Sabanjeff states that he, while preparing the iodide in this manner, got at the same time a large quantity of a liquid iodide (also of the formula $C_2H_2I_2$). He supposes the one to be  $\begin{array}{c}\mathrm{CHI}\\ \|\\ \mathrm{CHI}\end{array}$ and the other  $\begin{array}{c}\mathrm{CH_2.}\\ \|\\ \mathrm{CI_2.}\end{array}$ Curiously enough, I only obtained a few drops of a liquid which could at all correspond to this second iodide. Whether it was the iodide, or a product of the action of the iodine in the alcohol, &c., I cannot say. The quantity got was too small.

The chief object which I had in view in preparing the preceding acetylene compounds (chlor-iodide, iodide, also tetrabromide, &c.), was that from them I might be able to obtain the corresponding cyanogen ones, which, if got and treated with an alkali, would probably have yielded chloro- (or iodo-) acrylic, and fumaric or maleic acids. I have not, however, as yet succeeded in obtaining them.

Whether C_2H_2 combines with iodide of cyanogen when the latter is dissolved in alcohol, I cannot yet say. It does not with dry ICN.

All attempts to obtain the cyanogen compounds of C_2H_2 by heating in sealed tubes (at temperatures ranging from 120° to 0°25) chlor-iodide or iodide of acetylene with the various cyanides $\{$KCN, AgCN, (AgCN + KCN), $H_2(CN_2)$,$\}$ or with ICN and a metal, were fruitless.

When $H_2(CN)_2$ was used, generally no reaction occurred, and in nearly all the other cases ammonium salts were formed in large quantity. I had unfortunately only a small quantity of the chlor-iodide at my disposal.

Acetylene does not seem to be absorbed if passed into a hot solution of "nascent" formic acid, *i.e.*, a solution containing KCN and KOH.

4. On some Definite Integrals. By Professor Tait.

The integrals referred to occur in connection with applications of the Method of Electric Images. A curious special case was given to the Society in July 1875, but was not inserted in the "Proceedings." It depended on the fact that the image of a sphere, whose surface density is inversely as the cube of the distance from a point, is another sphere with a similar law of distribution. But any desired number of integrals, whose values can be at once assigned, may be obtained by various applications of the following process. Take any centrobaric distribution of electricity, and calculate directly the density induced by it at any point of an uninsulated sphere. This must be inversely as the cube of the distance from the centre of gravity of the given distribution.

Take, as a simple example, a uniformly charged sphere of non-conducting matter with unit charge, radius a, at a distance p from the centre of an uninsulated sphere of radius r. Suppose $r > a+p$, so that the inducing sphere is wholly internal. We see by the method above that the density of the induced charge at points defined by radii making an angle α with the line of centres is represented by either of the following expressions, multiplied by $\frac{1}{8\pi^2 r}$:—

$$\int_0^\pi\int_0^\pi \frac{(r^2-a^2-p^2+2ap\cos\theta)\sin\theta d\theta d\phi}{\left[r^2+a^2+p^2-2ap\cos\theta-2rp\cos\alpha+2ar\,(\cos\alpha\cos\theta-\sin\alpha\sin\theta\cos\phi)\right]^{\frac{3}{2}}}=\frac{2\pi(r^2-p^2)}{(r^2+p^2-2rp\cos\alpha)^{\frac{3}{2}}};$$

i.e., the double integral is independent of a.

Again, if the unit charge on the small sphere be distributed inversely as the cube of the distance from the centre of the large one, we have obviously for the induced density on the large sphere the expression—

$$\frac{a}{8\pi^2 r}\int_0^\pi\int_0^\pi \frac{(a^2-p^2)(r^2-a^2-p^2+2ap\cos\theta)\sin\theta d\theta d\phi}{\left[a^2+p^2-2ap\cos\theta\right]^{\frac{3}{2}}\left[r^2+a^2+p^2-2ap\cos\theta-2rp\cos\alpha+2ar\,(\cos\alpha\cos\theta-\sin\alpha\sin\theta\cos\phi)\right]^{\frac{3}{2}}}.$$

But if the small sphere *include* the centre of the large one, *i.e.*, if $a>p$, the induced density is uniform; so that the value of the integral is

$$\frac{2\pi}{ar}.$$

If $a < p$, similar reasoning shows that the value is

$$\frac{2\pi\left(r^2 - \left(p - \frac{a^2}{p}\right)^2\right)}{a\left(r^2 + \left(p - \frac{a^2}{p}\right)^2 - 2r\left(p - \frac{a^2}{p}\right)\cos a\right)^{\frac{3}{2}}}.$$

These values agree, as they should do, when $a = p$.

Monday, 1*st April* 1878.

Sir WILLIAM THOMSON, President, in the Chair.

The following communications were read:—

1. Chapters on the Mineralogy of Scotland. Chapter IV. Augite, Hornblende, and Serpentineous Change. By Professor Heddle.

(*Abstract.*)

A couple of months ago I had the honour of submitting to the Society a speculation upon the metamorphism of a particular rock mass. To-night I again return to metamorphism, submitting, however, not a speculation, but the closely elaborated process of the change which has affected another rock.

It is perhaps natural that the attention of one who approaches geology from the chemical and mineralogical sides should be immediately directed to those rocks which are either aggregates of simple minerals, or which are the products of changes effected *upon* simple minerals; natural also that consideration should be first given to those in which that change has been more immediately chemical than physical.

To no rock mass does this apply more directly than to serpentine. In my wanderings I have visited and closely observed the relationship of—with a single exception—every bit of this rock which is to be found between Unst and Tyree on the one hand,—Harris and the Black Dog Rock on the other.

In my analyses of a number of ill-defined minerals, generally believed to be products of the alteration of augite and hornblende,

the subject recurred to me. In order to bring as much light to bear upon it as possible, I greatly extended the number of analyses which I undertook; and, as bearing upon it, I have now analysed 20 specimens of augite and its allomorphs, 15 of hornblende, and 21 of the products of the alteration of these two substances.

Founding upon the information gained thereby, I work out chemically the various steps and stages of the transmutation,—which transmutation will be shown and seen to be the formation of serpentine.

2. On the Old Red Sandstone of Western Europe. By Professor Geikie, F.R.S.

In a historical introduction the author gives an outline of the progress of research into the history of the Old Red Sandstone of the British area. This system is at present regarded as composed of three sub-divisions, Lower, Middle, and Upper, each characterised by a distinct suite of organic remains. From the absence of unequivocally marine fossils, and from lithological characters, it has been inferred by Mr Godwin Austin, Professor Ramsay, Professor Rupert Jones, as well as other observers, and is now very generally admitted, that the Old Red Sandstone, as distinguished from the "Devonian" rocks, probably originated in inland sheets of water. The object of the present memoir was to endeavour to trace out in that geological system of deposits the changes of physical geography which took place over Western Europe during the interval between the close of the Upper Silurian and the beginning of the Carboniferous period.

After a sketch of the probable conditions of the region previous to the commencement of the Old Red Sandstone, the author proceeds to show how the shallowing Silurian sea was converted here and there into *salinas* or inland seas, by a series of subterranean movements, which have left their indelible traces upon the upturned Silurian rocks. He divides his memoir into two parts, the first dealing with the Lower and the second with the Upper Old Red Sandstone. The present paper deals only with a portion of the first of these sections. It traces out the limits of the different basins in which the Old Red Sandstone of the British islands were deposited, and for the sake of convenience, as well as briefness of reference, proposes short geographical names for these basins, which are arranged as follows:—

Area of the Basins.	Short reference names proposed to be applied to them.
1. The Old Red Sandstone tracts of the north of Scotland, embracing the region of the Moray Firth, Caithness, the Orkney Islands, the mainland of Shetland, and perhaps part of the south-western coast of Norway.	Lake Orcadie.
2. The central valley of Scotland between the Highlands on the north and the Silurian uplands on the south, including the basin of the Firth of Clyde, and ranging across the north of Ireland to the high grounds of Donegal.	Lake Caledonia.
3. A portion of the south-east of Scotland and north of England, extending from near St Abb's Head to the Head of Liddesdale, and including the area of the Cheviot Hills.	Lake Cheviot.
4. A district in the north of Argyllshire, extending from the mouth of the Sound of Mull to Loch Awe, and perhaps up into the southern part of the Great Glen.	Lake Lorne.
5. The Old Red Sandstone region of Wales and the border counties of England, bounded on the north and west by the older palæozoic hills, the eastern and southern limits being unknown.	The Welsh Lake.

Lake Orcadie.—After describing the limits of this basin, and giving a sketch of the labours of previous observers in the Old Red Sandstone tracts of the north of Scotland, the author proceeds to examine the evidence for the threefold arrangement of the Old Red Sandstone proposed by Murchison. He shows that nowhere are the three groups, Lower, Middle, and Upper, found in consecutive order; that this so-called "Middle" division occurs only in the north of Scotland, where it lies unconformably upon the older palæozoic rocks, and is itself unconformably overlaid by the Upper Old Red Sandstone, thus occupying a position exactly similar to that of the Lower Old Red Sandstone on the southern side of the Highlands. He further points out that while some species of fishes are common to the Old Red Sandstone on the two sides of the Highland barrier, the lithological differences between the deposits of the two areas are so great as to make it evident that the rocks were laid down in distinct basins, and consequently that the fauna of each basin might be expected to be more or less peculiar, as in many analogous cases at the present day. As evidence that adjacent areas in the time of the Lower Old Red Sandstone were strongly marked off from each other in their faunas, reference is made to the contrast between the fishes and crustaceans of the Welsh region and those of Lanarkshire and Forfarshire, not a single species being common to the two countries, though some of the genera are. Reasons are then given why the argument used by Murchison from the occurrence of many of the Scottish ichthyolites in Russia could not be regarded as establishing the existence of a "Middle" division of the Old Red Sandstone.

The conclusion arrived at by the author is that the Caithness flags or "Middle Old Red Sandstone" are probably the general equivalents of the Lower Old Red Sandstone of other regions, and that this system consists in Britain of two well-marked divisions only—a Lower, which graduates in some places into the Upper Silurian rocks, and is separated by an unconformability from an Upper, which in many districts passes up into the base of the Carboniferous system.

The various districts into which the area embraced under the term Lake Orcadie may be divided are then described *seriatim.* The detailed structure of Caithness has been worked out by the author (partly with the co-operation of his colleagues in the Geological Survey, Mr B. N. Peach and Mr John Horne) as affording the most complete sections of the Old Red Sandstone in the north of Scotland. Arranged in descending order, the various stratigraphical zones stand as in the subjoined table:—

	Thickness in feet.
9. John o' Groats red sandstones, flagstones, and impure limestones and shales,	2000
8. Huna flagstones, shales, and limestones,	1000
7. Gill's Bay red sandstones,	400
6. Thurso or northern group of flagstones, shales, and limestones,	5000
5. Wick or eastern group of flagstones, shales, and limestones passing down into red shales and sandstones,	5000
4. Dull red sandstones, red shales, and fine conglomerates,	2000
3. Brecciated conglomerates,	300
3. Badbea red sandstones and shales or clays,	450
1. Coarse basement conglomerates,	50
	16,200 ft.

From the four lowest sub-divisions no fossils have yet been obtained. The flagstones have yielded to Mr C. W. Peach and other observers many land plants (some of which resemble forms described by Dawson from the Gaspé sandstones) as well as *Estheria membranacea*, *Pterygotus*, sp., and many ichthyolites. Availing himself of the list of localities furnished to him by Mr Peach (to whom he cordially acknowledges his obligations), with the species of fish found at each, the author has constructed a table of the vertical distribution of the fossil fishes in Caithness. Some of the species range through almost the entire succession of beds. Some, however, are either peculiar to or very characteristic of one sub-division. Thus *Osteolepis arenatus* and *Dipterus Valenciennesi* are not noted except from the group No. 5. In the Thurso and the higher flagstones (Nos. 6, 8, and 9), *Acanthodes, Parexus, Cheiracanthus, Diplacanthus, Pterichthys*, and *Tristichopterus*—genera absent from the Wick beds—are found in greater or less abundance. These strata are further marked by peculiar species of genera which occur among the older flagstones, as *Coccosteus pusillus* and *Osteolepis microlepidotus.*

The Orkney Islands are assigned to the higher sub-divisions of the flagstone series, the protruding ridge of granite and gneiss which rises at Stromness and Gremsa being merely an indication of the irregular surface on which the deposits of Lake Orcadie were accumulated, and of the slow progressive subsidence of the area. The fossils, for which these islands have long been famous, include most of those of the upper groups of Caithness, with the addition of others which have been regarded as distinct. In the determination of these fossils, much skill is required to discriminate between the accidental differences of aspect resulting from the condition of fossilisation. The Orkney fishes, for instance, are preserved as black jet-like impressions which, often very perfect when first removed from the quarry, are apt to scale off, leaving in each case only an amorphous layer which, though it retains the contour of the fish, shows little or no trace of structure. On the shores of the Moray Firth, on the other hand, the organisms have been inclosed within calcareous nodules; their colours are sometimes brilliant, and their scales, plates, fins, and bones are often admirably preserved and remain unchanged in the Museum. Want of experience in these different modes of preservation may have led to a reduplication of species, especially in the case of the Orkney and Moray Firth fishes. Among the most interesting Orkney fossils is a portion of a *Pterygotus* (recognised by Dr H. Woodward), now in the British Museum. The occurrence there of this characteristically Upper Silurian and Lower Old Red Sandstone genus supports the view contended for in this paper as to the true horizon of the Orkney and Caithness flagstones.

The Shetland Islands contain a portion of the shore-line of Lake Orcadie, with its conglomerates and sandstones and the flagstones and shales of deeper water. Among these strata the Caithness *Estheria* occurs, with abundant stems and roots of large calamite-like plants with well-marked flutings, but without observable joints. Some ichthyolites of the Caithness type are said to have been found in Bressay. The general lithological characters are quite those of the sandy parts of the Orkney and Caithness groups. On the west side of the mainland of Shetland interesting evidence occurs to show the existence of volcanic action contemporaneous with the accumulation of the Old Red Sandstone. Beds of amygdaloidal lavas and bands of tuff occur among the sandstones, the whole being pierced by masses of pink felsite.

The south-western and southern margin of this great northern basin of the Old Red Sandstone can still be traced nearly continuously from the confines of Caithness to the borders of Aberdeenshire, its position being marked by a zone of littoral conglomerates. Beyond the edge of that zone, however, there occur some interesting outliers, which in some cases may represent long fjord-like indentations of the coast-line; in others may mark what were really independent basins lying at the base of the Grampian mountains. The author points out that probably most of the difficulty which has hitherto been experienced in understanding the sequence of beds along the southern shores of the Moray Firth, and their parallelism with those of Caithness and Orkney, is not to be attributed to the amount of detritus covering the country, but rather to the fact which has not heretofore been observed, that the Upper Old Red Sandstone, with *Holoptychius* and *Pterichthys major*, really overlaps unconformably upon the older nodular clays and conglomerates with *Coccosteus*, *Cheirolepis*, &c. This relation can be satisfactorily determined in Morayshire, and is now being worked out by Mr John Horne in the course of the Geological Survey. The author traces in great detail, from the Spey into Sutherlandshire, the development of the lower sandstones, conglomerates, and clays, which have been regarded as equivalents of the Caithness flagstones. He thinks that in no sense can this comparatively thin group of rocks (seldom 1400 feet in depth) be regarded as a mere southward attenuation of the great Caithness series, as suggested by Murchison, for that neither lithologically nor palæontologically can that view be sustained. He has been led to the conclusion that the whole of these rocks from the borders of Sutherlandshire to those of Aberdeenshire represent only the higher portions of the great Caithness series, and that they were formed during a gradual depression of the ancient high grounds whereby the waters of Lake Orcadie were allowed to creep southward over the descending land. This movement is indicated by the character of the strata, and that it took place about the time of deposit of the later flagstones of Caithness is shown by the occurrence of the fossils of that division in the nodules, flags, and clays of the Moray Firth region, while those of the Lower division are absent.

Allusion is likewise made to the discovery of two localities where contemporaneous volcanic action has recently been observed in the Moray Firth area, the whole of the basin of Lake Orcadie being otherwise remarkably free from any trace of such action except on the northern margin in Shetland. The history of the area embraced by Lake Caledonia will form the subject of the next paper.

3. On Beats of Imperfect Harmonies. By Sir William Thomson.

According to a usage which has been adopted from the German of Helmholtz by the best English scientific writers on sound, a sound is called a "simple tone,"* or without qualification a "tone," when the variation of pressure of the air in the neighbourhood of the ear which is the immediate excitant of the sense is according to a simple harmonic function of the time; that is to say, when the whole pressure of the air varies in simple proportion to the distance, from a fixed plane, of a point moving uniformly in a circle. Considering the actual sensibility of the human ear to musical sounds, we must introduce farther as a practical restriction that the period of the variation of the pressure must be less then $\frac{1}{30}$ of a second, and greater than $\frac{1}{10000}$ or $\frac{1}{20000}$ of a second. The vibrations of the air produced by a simple harmonic vibrator are either simple harmonic, or are in circular or elliptic orbits, resulting from the composition of two simple harmonic motions; and the consequent change of air-pressure in the neighbourhood of the ear follows the simple harmonic law, provided the maximum velocity of the vibrator and of the air in its neighbourhood be infinitely small in comparison with the velocity of sound. Hence the more nearly this condition is fulfilled the more nearly a simple tone is the sound heard; but it is far from being fulfilled when the vibrator, though itself performing simple harmonic motion, has sharp edges round which the

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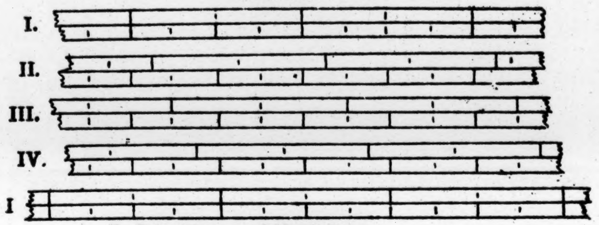
* The old musical usage, according to which the word tone denotes an interval (the major tone or minor tone, or the mean tone of the tempered scale), though it unfortunately clashes with this recent scientific use of the word tone, can scarcely be abandoned.

EVEN BINARY HARMONY.

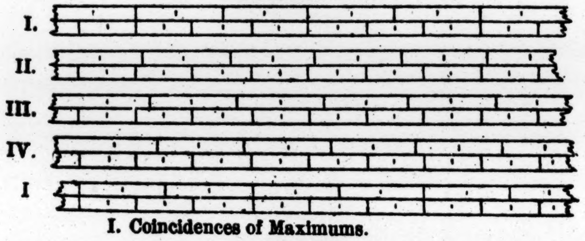
PERIODS 1 : 2.



I. Coincidences of Maximums.
„ „ Min. Max.
II. Phase relation at end of first quarter-period.
III. Phase relation at end of half-period.
Coincidences of Minimums.
„ „ Max. Min.
IV. Phase relation at end of third quarter-period.

EVEN BINARY HARMONY.

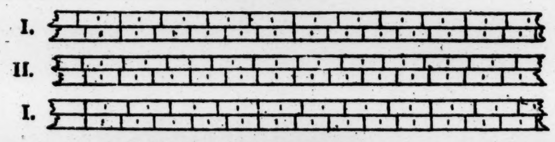
PERIODS 2 : 3.



I. Coincidences of Maximums.
„ „ Max. Min.
II. Phase relation at end of first quarter-period.
III. Phase relation at end of half-period.
Coincidences of Minimums.
„ „ Min. Max.
IV. Phase relation at end of third quarter-period.

EVEN BINARY HARMONY.

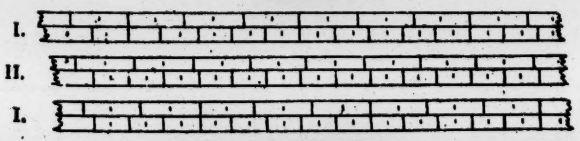
PERIODS 4 : 5.



I. Coincidences of Maximums.
„ „ Max. Min.
II. Phase relation at end of half-period.
Coincidences of Minimums.
„ „ Min. Max.

ODD BINARY HARMONY.

PERIODS 3 : 5.

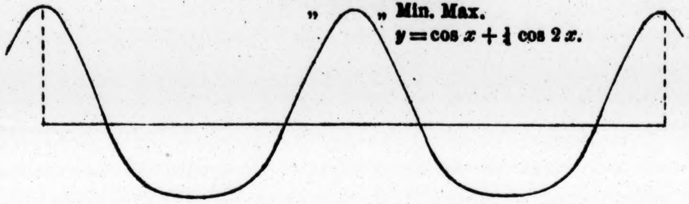


I. Coincidences of Maximums.
„ „ Minimums.
II. Phase relation at end of half-period.
Coincidences of Max. Min.
„ „ Min. Max.

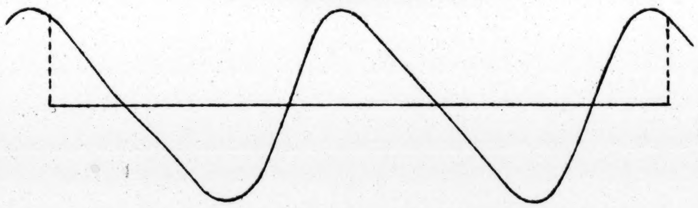
EVEN BINARY HARMONY.

PERIODS 1 : 2.

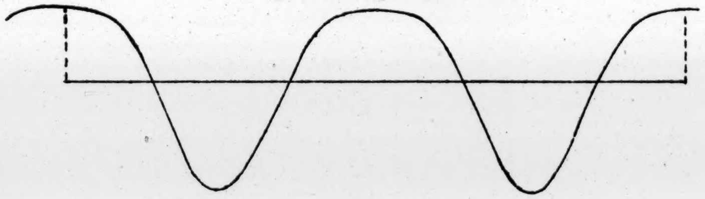
I. Coincidences of Maximums.
„ „ Min. Max.
$y = \cos x + \frac{1}{2} \cos 2x.$



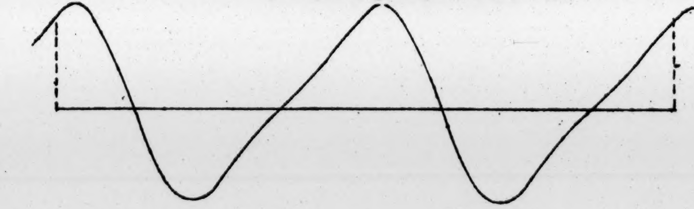
II. Phase relation at end of first quarter-period.
$y = \cos x + \frac{1}{2} \cos (2x + 90°).$



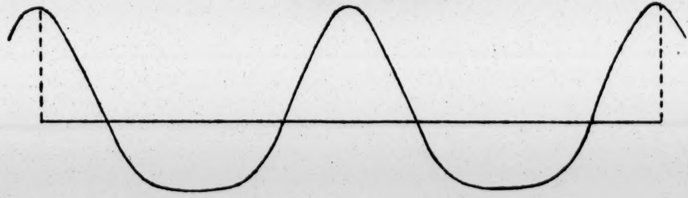
III. Phase relation at end of half-period.
Coincidences of Minimums.
„ „ Max. Min.
$y = \cos x + \frac{1}{2} \cos (2x + 180°).$



IV. Phase relation at end of third quarter-period.
$y = \cos x + \frac{1}{2} \cos (2x + 270°).$



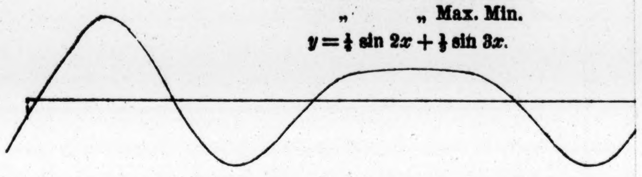
I. Phase relation at end of whole period.
$y \cos x + \frac{1}{2} \cos 2x.$



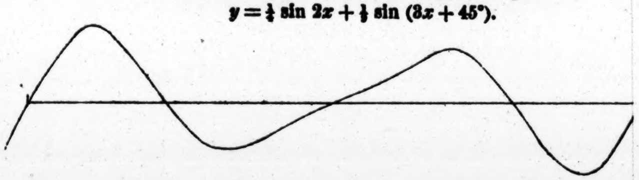
EVEN BINARY HARMONY.

PERIODS 2 : 3.

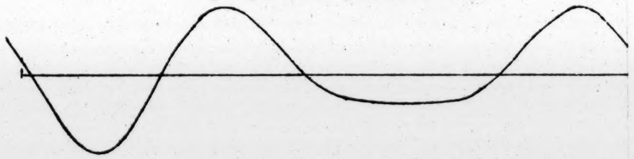
I. Coincidences of Maximums.
„ „ Max. Min.
$y = \frac{1}{2} \sin 2x + \frac{1}{3} \sin 3x.$



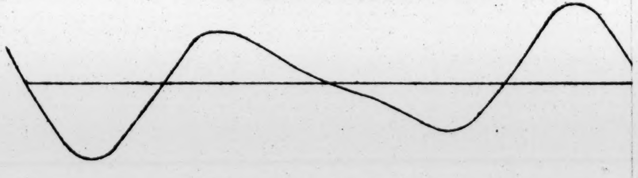
II. Phase relation at end of first quarter-period.
$y = \frac{1}{2} \sin 2x + \frac{1}{3} \sin (3x + 45°).$



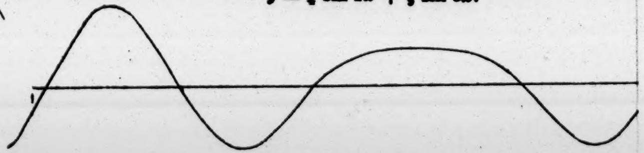
III. Phase relation at end of half-period.
Coincidences of Maximums.
„ „ Min. Max.
$y = \frac{1}{2} \sin 2x + \frac{1}{3} \sin (3x + 90°).$

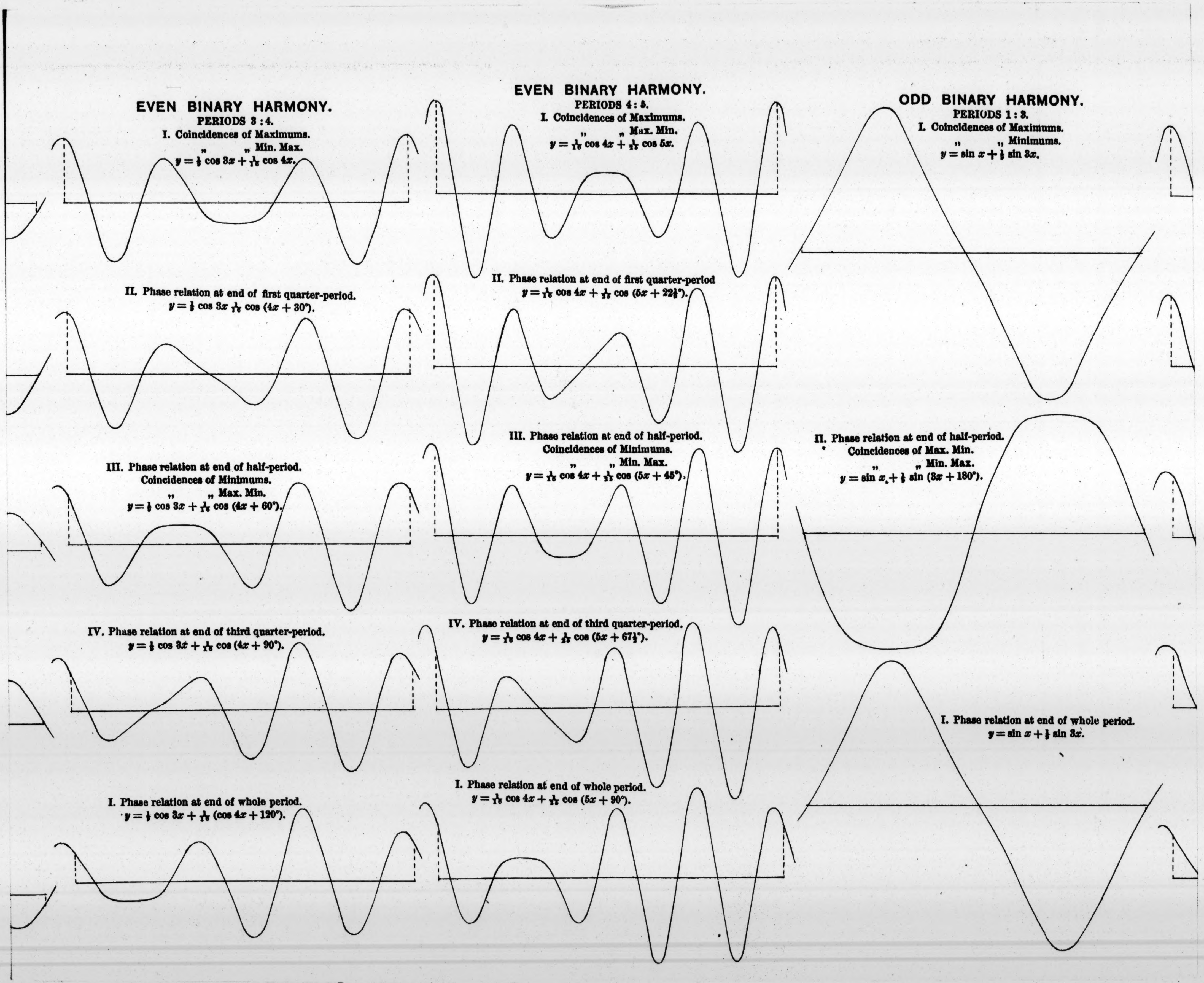


IV. Phase relation at end of third quarter-period.
$y = \frac{1}{2} \sin 2x + \frac{1}{3} \sin (3x + 135°).$



I. Phase relation at end of whole period.
$y = \frac{1}{2} \sin 2x + \frac{1}{3} \sin 3x.$





EVEN BINARY HARMONY.

PERIODS 3 : 4.

I. Coincidences of Maximums.
„ „ Min. Max.
$y = \frac{1}{9} \cos 3x + \frac{1}{16} \cos 4x$.

II. Phase relation at end of first quarter-period.
$y = \frac{1}{9} \cos 3x\ \frac{1}{16} \cos (4x + 30°)$.

III. Phase relation at end of half-period.
Coincidences of Minimums.
„ „ Max. Min.
$y = \frac{1}{9} \cos 3x + \frac{1}{16} \cos (4x + 60°)$.

IV. Phase relation at end of third quarter-period.
$y = \frac{1}{9} \cos 3x + \frac{1}{16} \cos (4x + 90°)$.

I. Phase relation at end of whole period.
$y = \frac{1}{9} \cos 3x + \frac{1}{16} (\cos 4x + 120°)$.

EVEN BINARY HARMONY.

PERIODS 4 : 5.

I. Coincidences of Maximums.
„ „ Max. Min.
$y = \frac{1}{16} \cos 4x + \frac{1}{25} \cos 5x$.

II. Phase relation at end of first quarter-period
$y = \frac{1}{16} \cos 4x + \frac{1}{25} \cos (5x + 22\frac{1}{2}°)$.

III. Phase relation at end of half-period.
Coincidences of Minimums.
„ „ Min. Max.
$y = \frac{1}{16} \cos 4x + \frac{1}{25} \cos (5x + 45°)$.

IV. Phase relation at end of third quarter-period.
$y = \frac{1}{16} \cos 4x + \frac{1}{25} \cos (5x + 67\frac{1}{2}°)$.

I. Phase relation at end of whole period.
$y = \frac{1}{16} \cos 4x + \frac{1}{25} \cos (5x + 90°)$.

ODD BINARY HARMONY.

PERIODS 1 : 3.

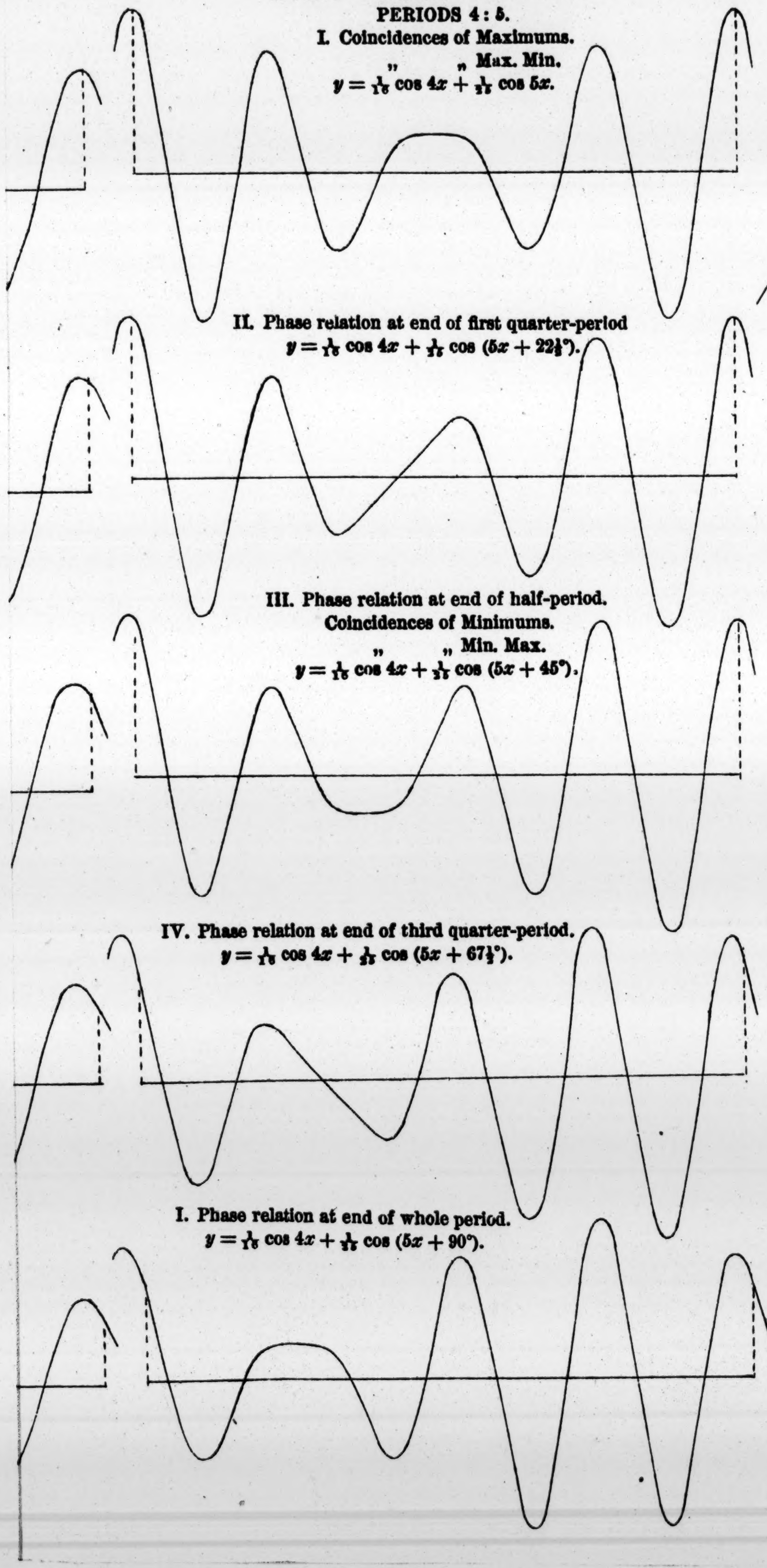
I. Coincidences of Maximums.
„ „ Minimums.
$y = \sin x + \frac{1}{9} \sin 3x$.

II. Phase relation at end of half-period.
Coincidences of Max. Min.
„ „ Min. Max.
$y = \sin x + \frac{1}{9} \sin (3x + 180°)$.

I. Phase relation at end of whole period.
$y = \sin x + \frac{1}{9} \sin 3x$.

EVEN BINARY HARMONY.

PERIODS 4 : 5.



I. Coincidences of Maximums.
" " Max. Min.
$y = \frac{1}{16} \cos 4x + \frac{1}{25} \cos 5x.$

II. Phase relation at end of first quarter-period
$y = \frac{1}{16} \cos 4x + \frac{1}{25} \cos (5x + 22\frac{1}{2}°).$

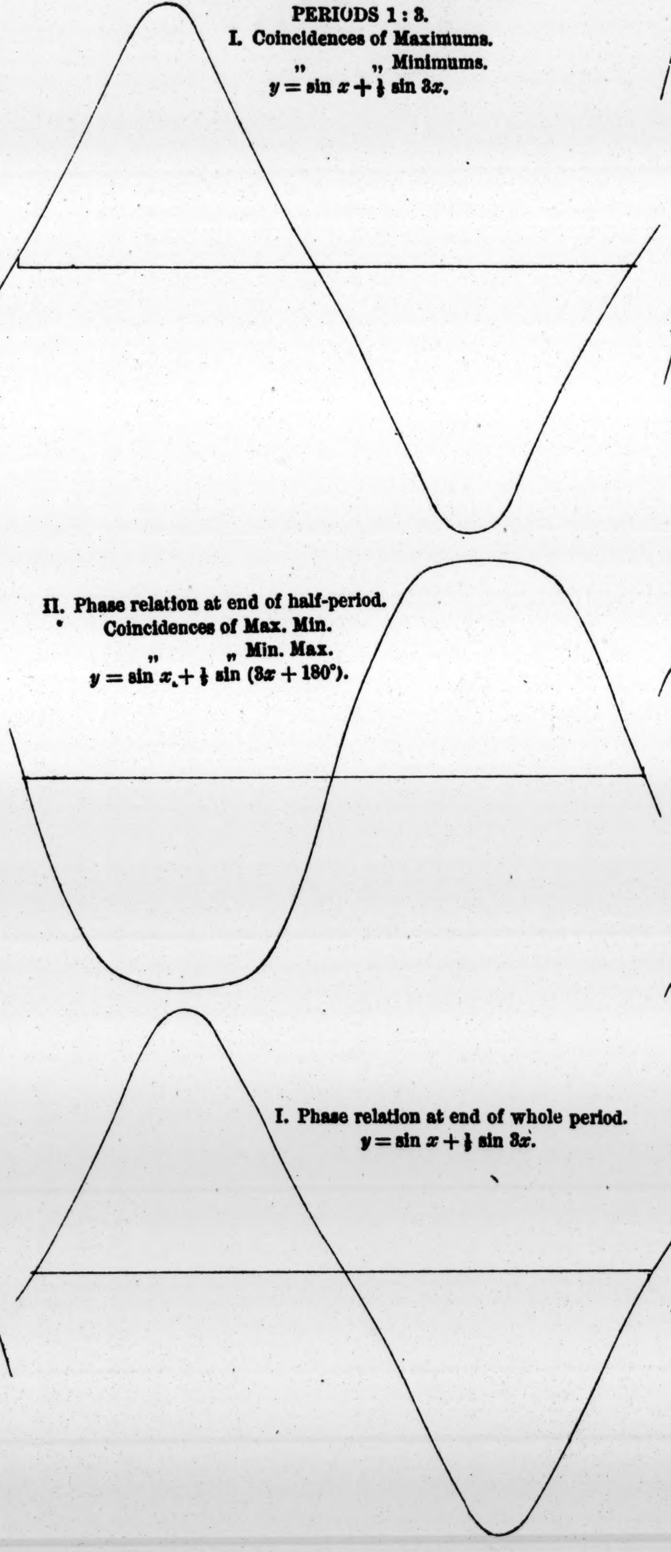
III. Phase relation at end of half-period.
Coincidences of Minimums.
" " Min. Max.
$y = \frac{1}{16} \cos 4x + \frac{1}{25} \cos (5x + 45°).$

IV. Phase relation at end of third quarter-period.
$y = \frac{1}{16} \cos 4x + \frac{1}{25} \cos (5x + 67\frac{1}{2}°).$

I. Phase relation at end of whole period.
$y = \frac{1}{16} \cos 4x + \frac{1}{25} \cos (5x + 90°).$

ODD BINARY HARMONY.

PERIODS 1 : 3.



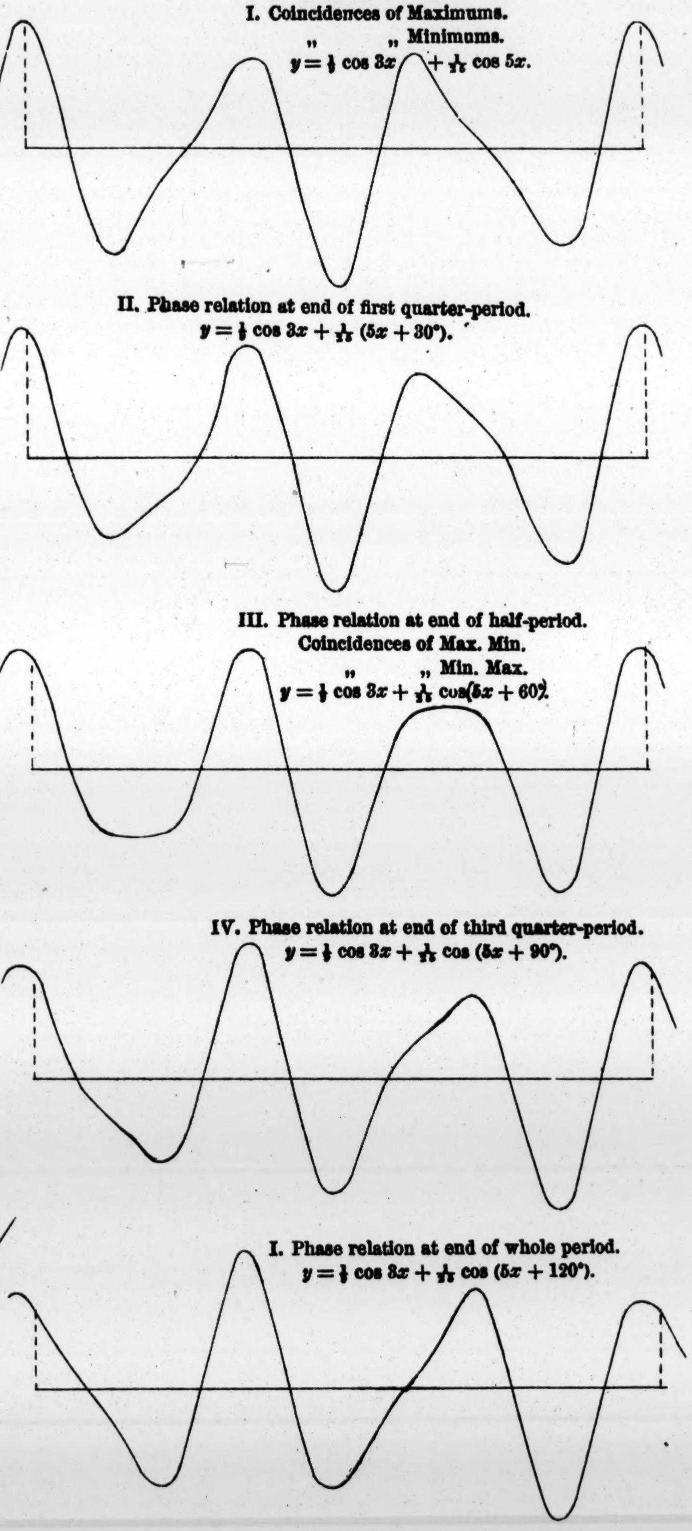
I. Coincidences of Maximums.
" " Minimums.
$y = \sin x + \frac{1}{9} \sin 3x.$

II. Phase relation at end of half-period.
Coincidences of Max. Min.
" " Min. Max.
$y = \sin x + \frac{1}{9} \sin (3x + 180°).$

I. Phase relation at end of whole period.
$y = \sin x + \frac{1}{9} \sin 3x.$

ODD BINARY HARMONY.

PERIODS 3 : 5.



I. Coincidences of Maximums.
" " Minimums.
$y = \frac{1}{9} \cos 3x + \frac{1}{25} \cos 5x.$

II. Phase relation at end of first quarter-period.
$y = \frac{1}{9} \cos 3x + \frac{1}{25} (5x + 30°).$

III. Phase relation at end of half-period.
Coincidences of Max. Min.
" " Min. Max.
$y = \frac{1}{9} \cos 3x + \frac{1}{25} \cos (5x + 60°).$

IV. Phase relation at end of third quarter-period.
$y = \frac{1}{9} \cos 3x + \frac{1}{25} \cos (5x + 90°).$

I. Phase relation at end of whole period.
$y = \frac{1}{9} \cos 3x + \frac{1}{25} \cos (5x + 120°).$

air is forced to rush with great velocity, or when, as in the case of free-reed organ pipes or the reeds of a harmonium, the vibrator is an elastic solid moving to and fro in a very narrow aperture. (In the case of a slapping reed, as of trumpet stops in an organ, the motion of the vibrator itself is not simple harmonic, and the sound is excessively rich in overtones, giving it its peculiarly rich or harsh character.)

A harmony is any sound of which the excitant change of air-pressure is strictly periodic, and is not a simple tone. According to Fourier's beautiful analysis* of periodic variations, to which the name of the harmonic analysis has been given, any periodically varying quantity may be regarded as the sum of quantities varying separately according to the simple harmonic law, in periods respectively equal to the main period, half the main period, a third of the main period, and so on. According to this analysis we see that the variation of air-pressure constituting a harmony may be regarded as the sum of variations constituting simple tones, one having its period equal to the period of the harmony; a second, half that of the harmony; a third, one-third that of the harmony, and so on; in other words, we may regard the harmony as compounded of these simple tones.

Practically, in musical language the term harmony is not applied when the tone of the main period predominates in the sensory impression, and in this case the sound is simply called a note; its pitch is reckoned according to the main period; and the effect of the other tones, now called overtones, which enter into its composition, are merely felt as giving it its character or quality of sound. Thus the name harmony is in musical practice restricted to cases in which there is either no tone of the main or fundamental period, or not enough to produce a predominating impression, and a sound compounded of two, three, four, or more simple tones, having commensurable periods, is heard. In ordinary musical language a harmony is not regarded as having any one pitch, but is thought of as compounded of its known constituents. The true period of the harmony is, however, in every case the least common multiple of the period of its constituent tones. The number of times that the period of the harmony contains the period of any one of its constituent tones is called the harmonic number of that tone. This expression is only applicable to any particular tone when viewed as one constituent of a harmony. Following the usage of Lord Rayleigh and Professor Everett, I shall employ the word "frequency" to denote the number of periods per unit of time,—per second, let us say, generally in acoustical reckonings. Thus the "frequency" of a tone or of a harmony means the number of its periods per second. Similarly the frequency of any set of beats, according to the definitions and descriptions below, will mean the number of the beats per second, and in this application of the term it will designate sometimes a proper fraction, and sometimes a small whole number with fraction.

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* Compare "Trans. R.S.E." April 30th, 1860, "Reduction of Observations of Underground Temperature," where a short description of Fourier's analysis is to be found.

The quality of a harmony, when the periods of its several constituent tones are given, depends upon the amplitudes of the different constituents, and on the relation of their phases. Thus, for example, consider a harmony of two tones. They may be so related in phase that at one of the instants of maximum pressure of one of the constituents there is also maximum pressure of the other constituent. The same phase-relation, if the harmonic numbers of the constituent tones be both odd, will give also coincident minimums. But when one of the harmonic numbers is even and the other odd the phase-relation of coincident maximums will also be such that there is a coincidence of minimum pressure due to one tone with maximum pressure due to the other; and again there will be an opposite phase in which there will be coincidence of minimums, and in this opposite phase there will also be a coincidence of maximum and minimum. (To avoid circumlocutions a harmony of two odd numbers will be called an odd binary harmony; a harmony of even and odd numbers will be called an even binary harmony.) Thus we see that in an odd binary harmony there is a phase-relation of coincident maximums and coincident minimums, and again an opposite phase-relation of coincident maximum minimum and minimum maximum. The former will be called the phase-relation of coincidences, the latter the phase-relation of oppositions. In an even binary harmony there is a phase-relation of coincident maximums and coincident maximum minimum; and again an opposite phase-relation of coincident maximum minimum and coincident minimums. The former will be called the phase-relation of coincident maximums, the latter the phase-relation of coincident minimums. The annexed diagrams illustrate and prove these assertions. The horizontal line in each may be either regarded as representing space at any one instant in the direction of propagation of the sound, or it may represent times at which successive phases of the motion are perceived by an ear in a fixed position. The long vertical cross-bar in each case denotes maximum of air-pressure; the short vertical cross-bar, minimum.

For lecture illustrations it is convenient to use long slips of wood with paper pasted on one side, and the short and long cross-bars marked upon it, and to support these slips of wood on a board with nails to guide them so that they may be placed in groups of two, three, or four, one over the other, and any of them moved in the direction of its length to illustrate the different phase-relations of a harmony.

Suppose now, one note of a perfect binary harmony to be very slightly sharpened or flattened: so slightly that during a large number of the periods of the perfect harmony, the phase-relation in the imperfect harmony experiences but little change. Let the two notes of the imperfect harmony be sustained long enough with perfect uniformity as to pitch and intensity:—the effect will be that of perfect harmony, modified by a slow change of its phase-relation through a cycle; which in the case of an even binary harmony is from coincident maximums gradually to coincident minimums, and thence gradually round again to coincident minimums; and in the case of an odd binary harmony is from oppositions to coincidences, and round to oppositions again; and so on in cycles. In favourable circumstances, and with careful attention, a variation of the quality of the sound recurring periodically in these successive cycles is distinctly heard, even by an unpractised ear, unless the duration of the cycle be too long or too short to suit its sensibility. It is this variation which is called the "beat" on the imperfect harmony.

The period of the beat—that is to say, the duration of the cycle described above—is most easily found by taking the reciprocal of its frequency, calculated by the following rule:—*The frequency of the* *beat is equal to the error of frequency of one note multiplied by the harmonic number of the other.* When in a harmony of three or four notes all are perfect except one, the beats due to the imperfection of the false one are to be reckoned just as if the harmony were binary, according to the following rule :—

For the two or more notes which are in perfect harmony imagine one whose period is the period of their harmony. Take this as if it were one tone of an approximate binary harmony, the false note of the given harmony being the other. Example: Let the frequencies of the three notes be 257, 320, and 384: the common period of the two last-mentioned is $\frac{1}{64}$ of a second, and we have to calculate the beats on two notes whose frequencies are 64 and 257. The harmonic numbers of the harmonies to which these notes approximate are 1 and 4, and the error in frequency of the higher note is 1 per second; hence the beats are at the rate of 1 per second. When there is error in two or more notes of a multiple harmony, two or more sets of beats in periods not commensurable with one another are heard; but the general effect is apt to be too confused to allow any one of the sets to be distinctly counted. On a multiple harmony with only one note false the beats are in general exceedingly distinct, more so in general than in binary harmonies.

Sometimes, as for distance in reckoning the beats in the imperfect harmonies of a tempered musical scale, it is convenient to regard the two notes of an imperfect harmony as in error from two notes of a perfect harmony differing but little from them; then the rule for calculating the frequency of the beats is to take the difference of the products of the errors of the two notes, each multiplied into the harmonic number of the other. Thus, let n and n' be the harmonic numbers of the perfect harmony to which the given notes approximate, and let e and e' be the excesses of the vibrational frequencies of the two actual notes above two in perfect harmony nearly agreeing with them. The frequency of the beat of the actual notes is $n'e - ne'$.

For example, take the following table of numbers of vibrations in a perfect diatonic scale, with 256 vibrations per second for C, and in the corresponding scale of equal temperament (founded on 12 equal semitones, in each of which the interval ratio is $2^{\frac{1}{12}}$) :—

Errata to be corrected in Table at top of page 607 *opposite.*

Column 1, second row of figures, *for* "286·75," *read* "288."
,, ninth ,, ,, "573·50," ,, "576."
Column 3, second ,, ,, "+0·60," ,, "−0·65."
,, ninth ,, ,, "+1·20," ,, "−1·30."

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Frequencies of True.	Frequencies of Tempered.	Error of Tempered.
C 256	256 C	0·0
D 286·75	287·35 𝕯	+0·60
E 320	322·54 𝕰	+2·54
F 341·33	341·72 𝕱	+0·39
G 384	383·57 𝕲	−0·43
A 426·67	430·54 𝕬	+3·87
B 480	483·26 𝕭	+3·26
C′ 512	512 C′	0·0
D′ 573·50	574·70 𝕯′	+1·20
E′ 640	645·08 𝕰′	+5·08

From these numbers we find the following table of beats :—

Perfect Harmonies.	Harmonic Numbers.	Imperfect Harmonies.	Qualities of Falseness of the Intervals.	Frequencies of Beats.
		Errors.		
G C	3 2	𝕲 − 0·43 C 0·0	Too small	0·86
B E	3 2	𝕭 + 3·26 𝕰 + 2·54	Too small	1·10
C′ F	3 2	C′ 0·0 𝕱 + 0·39	Too small	1·17
E′ A	3 2	𝕰′ + 5·08 𝕬 + 3·87	Too small	1·45
F C	4 3	𝕱 + 0·39 C 0·0	Too large	1·17
A E	4 3	𝕬 + 3·87 𝕰 + 2·54	Too large	1·45
E′ B	4 3	𝕰′ + 5·08 𝕭 + 3·26	Too large	2·20
E C	5 4	𝕰 + 2·54 C 0·0	Too large	10·16
A F	5 4	𝕬 + 3·87 𝕱 + 0·39	Too large	13·53

Perfect Harmonies.	Harmonic Numbers.	Imperfect Harmonies. Errors.	Qualities of Falseness of the Intervals.	Frequencies of Beats.
G E	6 5	𝕲 − 0·43 𝕰 + 2·54	Too small	17·39
G E	6 5	G 0·0 𝕰 + 2·54	Too small	15·24
C′ A	6 5	C′ 0·0 𝕬 + 3·87	Too small	23·22
A C	5 3	𝕬 + 3·87 C 0·0	Too large	11·61
E′ G	5 3	𝕰′ + 5·08 𝕲 − 0·43	Too large	17·39
E G C	5 2	𝕰 + 2·54 G 0·0 C 0·0	Too large	5·08

It is of course to be understood that the degree of falseness is the same in all the tempered harmonies of the same name (or having the same harmonic numbers); and that the different numbers shown for the frequencies of the beats are (except for the case of the 𝕰 with the untempered G) in simple proportion to the vibrational frequencies of one or other of the constituent notes. The slightness of the imperfectness in the tempered fifth (approximately 2 : 3) is indicated by the slowness of the beats, not so much as one per second on the C 𝕲. The imperfectness of the fourth (approximately 3 : 4) is even less than that of the tempered fifth, so that, notwithstanding the greater harmonic numbers, the beats are scarcely more rapid (1·17) for the C 𝕱 than (·86) for the C 𝕲. But when we go to major and minor thirds of the tempered scale, we take leave of mathematical harmony entirely. The beats on the C 𝕰 (ten per second) are too rapid to be counted, and it is only in virtue of their not being perceived, or not being disagreeably perceived, that the combination is agreeable. The same may be said still more unqualifiedly of the minor thirds, the number of beats on 𝕰 𝕲 being more than seventeen per second. It does not seem easy to explain on any physical or physiological principles the decidedly agreeable effect produced on the ear by a succession of major and minor thirds of pianoforte notes. It is, no doubt, to the slowing of the beats by the superposition of a third note upon either of the binaries C 𝕰 or 𝕰 𝕲 in the ternary combination C 𝕰 𝕲, because of its comparatively close approximation to $\overset{\frown}{\mathrm{C\ G}}$ 𝕰 (for which the beats are only five per second), that the comparatively smooth harmoniousness of the common chord in the tempered scale is due.

It is not generally known how easily beats on approximations to other harmonies than unison are heard, even when the constituent notes are simple tones. Through the kindness of Professor M'Kendrick I have been allowed the means of testing them in very varied combinations, by aid of a series of excellent tuning-forks of Koenig's, each mounted on a wooden box resonator, after the manner of Marloye. For such experiments Koenig's tuning-forks are much superior to Marloye's, because of the greater quantity of metal in each fork, in virtue of which it gives a louder and more enduring sound. The sound proceeding from such a source is essentially a simple tone, or very nearly so. I have tested that in every case the number of beats counted is the smallest that could be according to the preceding theory; for it is to be remarked that the theory only gives the whole period of the phenomenon, but does not answer the question—Does the ear perceive a gradual variation of quality through the whole period, or does it fail to distinguish the difference of quality between two halves of the period, or between three-thirds of it, and so on? My experiments demonstrate that in every case the ear does distinguish the two halves of the period of each beat. Thus, for example, in the beat on an approximation to the harmony (1 : 2) in which the variation of air-pressure on the ear is represented by the preceding curves for four instants of the period noted, I find that the ear distinguishes the quality of the sound represented by the sharp-topped and flat-hollowed curve from that represented by the flat-topped and sharp-hollowed curve. In the one case the pressure of air close to the ear rises very suddenly to, and falls very suddenly from, its maximum, and (as in cases of tides in which there is a long hanging on low water) there is a comparatively slow variation of pressure for a few ten-thousandths of a second on each side of the instant of minimum pressure; in the opposite phase-relation there is a slow change before and after the time of maximum pressure, and a rapid change before and after the time of minimum pressure. In the former case the difference of maximum from mean exceeds the difference of minimum from mean; in the latter the difference of the minimum from mean exceeds the difference of maximum from mean. If the ear could not distinguish between two such sounds, but could distinguish between either of them and the sounds represented by the first and third quarter-period curves, the number of beats would be twice the error of frequency of the higher note. But I find that the number of beats is quite distinctly equal to the error of frequency of the higher note. I have found that the beats on the 1 : 2 harmony are most easily perceived when the intensity of the higher note is comparatively faint, as would be the case if they were explained by Helmholtz's theory that they are the beats on the approximation to unison which there is between the higher note and the first overtone of the lower note. But the simple-harmonic character of the two constituent tones at the entrance of the ear precludes the acceptance of this theory unless extended, as it has actually been by its author, to the interior mechanism of the ear.

Whatever may be the physiological theory by which the beats are to be explained, it is an interesting fact that the ear does distinguish, as it were, between push and pull on the tympanum in the manner illustrated by the preceding curves, not only for the case of approximation to the harmony 1 : 2, but for every other even binary harmony. I have heard distinctly the beats on approximations to each of the harmonies 2 : 3, 3 : 4, 4 : 5, 5 : 6, 6 : 7, 7 : 8, 1 : 3, and 3 : 5. The two last mentioned, though sometimes less easily heard than the beats on most of the others, are unmistakably distinct; and by counting the numbers of them in ten seconds or in twenty seconds, I have ascertained that they, as do all the others, fulfil the condition of having the whole period of the imperfection, and not any sub-multiple of it for their period of audible beat. They are interesting as being cases of odd binary harmonies. Before making the experiments, I thought it possible that what is heard in the beat might not make distinction between the configuration II. and IV. (first quarter phase and third quarter phase): but a *revolving character* which I perceive in the beat seems to me distinct enough to prove that the ear does distinguish between these configurations, which are one of them the same as the other taken in the reverse order of time.

In every instance except the octave, the beat on the approximation to a binary harmony is less distinct than the beat on an approximation to a ternary or higher multiple harmony with only one note false. It is not because of the comparative slowness of the beat on the multiple harmony; for by taking alternately beats with one note slightly false in a binary harmony, and the same note made more false in a ternary or multiple harmony to such a degree as to give the same number of beats, I have always found the beats in the latter case much more prominent than in the former. Thus by taking first the perfect harmony C E G (4, 5, 6), and the three binary harmonies C G (2 : 3), C E (4 : 5), E G (5 : 6), and flattening slightly any one of the three notes by screwing on a small mass of brass to either or to each prong of the tuning-fork producing it, it is easy after a little practice to count the beats on each of the binary harmonies. Thus, for example (supposing E$_{,}$ to designate a note of a slightly lower pitch than E), after a little practice it is easy to count the beats on C E$_{,}$ and on the E$_{,}$ G, and to verify that their frequencies are, the first of them four times, and the second of them six times the error of frequency of the E$_{,}$, and then to verify that the much louder beats on the ternary harmony C E$_{,}$ G, are of half the frequency of the former, and of one-third of the frequency of the latter, and to verify absolutely that they are of twice the frequency of the error of E$_{,}$.

If when the approximate harmony C E$_{,}$ is being sounded, the faintest sound of G is produced by a very gentle excitation of the fork by the bow, instantly a loud beat at half speed is heard. The phenomenon is rendered very striking by alternately touching the top of the G fork by the bow so as to stop its vibrations, and then drawing the bow very gently for a fraction of a second* along one side to re-excite them. It is marvellous how small an intensity of the sound G is required to give a smooth unbroken loud beat in the double period. I have found it difficult to excite

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[*] In every case, to obtain regular beats, each tuning-fork, after being set in vibration by the bow, must be left to itself. The sound is sensibly graver as long as the bow is applied to augment or sustain the vibration than when the fork is left free. Thus, if two tuning-forks nearly, but not quite, in unison, are alternately acted on by the bow and left free, the beats are less rapid during the time the bow is applied to the higher fork, and more rapid while to the lower, than when both forks are vibrating freely.

the G gently enough to give the gradual transition from, let us say for example, four uniform beats per second, through the case of four beats per second with every alternate beat somewhat louder, to the case of only every second beat perceptible, or, in all, two beats per second; but it can be done, and the result is an interesting and instructive illustration of the slowing down from the quick beat of the binary harmony to half speed, or to one-third speed, or to one-fifth speed, as the case may be, by the introduction of a third note. In the several cases I have found that I can, by making the added note faint enough, produce a succession of beats of which every second, or every third, or every fifth, as the case may be, is louder than the others, and that, as the intensity of the added note is gradually increased, the fainter beats become imperceptible, and a regular unbroken slow beat is heard distinctly alone, always in the theoretical time of the whole imperfection of the harmony. I have verified this distinctly in the cases of 1, 2, 3; 2, 3, 4; 3, 4, 5; 4, 5, 6 (as stated above); 5, 6, 7; and 6, 7, 8. I have not succeeded in hearing the beats on the approximations to the harmonies 8 : 9 and 9 : 10. But the slow beat on the 8, 9, 10 (with vibrational frequencies 256, 288, 320), with any one of the three notes slightly flattened, is very remarkable. The sound is like that of a wheel going round with decided roughness of motion in every part of its revolution, but much rougher in one part than another, with a loudly perceptible periodic return of the roughness in the theoretical period of the approximate harmony.

The beats on the harmony C E G (vibrational frequencies 256, 320, 384), with any one of the three notes slightly flattened, are very perceptible: untrained ears hear them instantly the first time without any education, and the beat is heard almost to the very end of the sound if three of Koenig's forks, one of them, the C, for example, being slightly flattened by a brass sliding piece screwed to it, be caused to sound. The sound dies beating, the beats being distinctly heard all through a large room as long as the faintest breath of the sound is perceptible. The smooth melodious periodic moaning of the beat is particularly beautiful when the beat is slow (at the rate, for instance, of one beat in two seconds or thereabouts), being, in fact, sometimes the very last sound heard when the intensities of the three notes chance at the end to be suitably proportioned.